AN

INQUIRY

INTO THE NATURE AND PROPERTIES OF

OPIUM.

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INTO THE NATURE AND PROPERTIES OF

OPIUM;

WHEREIN

ITS COMPONENT PRINCIPLES,
MODE OF OPERATION, AND
USE OR ABUSE IN PARTICULAR DISEASES,

ARE EXPERIMENTALLY INVESTIGATED; AND THE OPINIONS OF FORMER AUTHORS ON THESE POINTS IMPARTIALLY EXAMINED.

BY SAMUEL CRUMPE, M. D.

MEMBER OF THE ROYAL IRISH ACADEMY.

Hoc ipsum *prius* et *posterius* in omni actione naturali notari debet.
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MDCCXCIII.

DEDICATION.

TO

JAMES GREGORY, M. D.

PROFESSOR OF THE PRACTICE OF PHYSIC IN THE UNIVERSITY OF EDINBURGH, &c. &c.

DEAR SIR,

IF the acknowledgments due to a name, not uncelebrated in the annals of philoſophy and medicine; if the remembrance of pleaſure at once, and information de-

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rived from an inſtructor, and of obligations experienced from a friend, be motives ſufficient to warrant a Dedication; to no character can the ſucceeding pages be inſcribed with greater propriety than to yourſelf. Accept this addreſs therefore as the genuine tribute of reſpect, eſteem, and gratitude, from an obliged pupil,

THE AUTHOR.

PREFACE.

To intrude upon the medical world opinions in any degree novel, would ſeem to demand apology; as the writer who promulgates them aſſumes, in ſome degree, from the very nature of publication, the taſk of inſtructing others; and as his opinions, if erroneous, are likely to produce more ſerious and practical evils in the ſcience of medicine, than in any other branch of human knowledge. So conſcious of theſe truths was the author of the ſucceeding pages, that he deemed it a duty, previous to their being ventured into public, to lay them before a few friends, for whoſe opinions he himſelf entertained no ſmall veneration; and before others, whoſe

 cha-

characters have been long and justly respected in medical science. The sentiments which each expressed concerning the Work it is not necessary to repeat. It is hoped the subsequent extract of a Letter from Doctor Gregory of Edinburgh, respecting its merits, will sufficiently excuse the publication of the performance, will sufficiently screen its author from the imputation of giving publicity to a composition uninteresting or erroneous.

"A few weeks ago Doctor B. put into "my hands your MS. on Opium. I have "read it with attention, and have told "him with great sincerity, as I now tell "yourself, that I think it a work of con-"siderable merit, the publication of "which will do you credit with all who "shall read and understand it. Your "book exhibits the result of much study, "both in point of reading and thought. "Your reasonings are acute, and your "observations and conclusions are im-

" portant, and for the moſt part, I really " believe, juſt and fairly deduced. Your " Experiments too are well contrived, " and diſtinctly told, and ſeem to have " been very carefully conducted: as a " book of medical ſcience, therefore, I " have no doubt that your work poſſeſſes " conſiderable merit."

This Letter is dated the 11th of April, 1792, at which time the work was completed in its preſent form. A variety of accidents, immaterial to mention, have deferred its publication to the preſent period.

Limerick,
November, 1793.

ERRATA.

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INTRO-

INTRODUCTION.

THAT the advancement of any branch of ſcience can never be retarded, either by a free inquiry into principles, however univerſally and implicitly adopted, or by the promulgation of new opinions, however ill founded, if they alſo be impartially examined, are poſitions which will appear, I imagine, upon a little conſideration, ſufficiently evident. For if the former be founded in truth, they will ſecurely ſtand the teſt, and be more fully confirmed by the cloſeſt inveſtigation; if erroneous, no greater benefit can be conferred upon the cauſe of ſcience, than detecting their errors and defects. If, on the contrary, new ſyſtems be broached, the offspring rather of a creative imagination and warm fancy,

than of accurate experiment and clear deduction; their fallacy will ſoon be detected and expoſed, and their publication prove injurious to the author alone.

Theſe ſimple conſiderations are alone ſufficient to evince the truth of what has been advanced; but we may go ſtill farther, and ſhall be equally well juſtified in aſſerting, that freedom of inquiry, and the unreſtrained publication of its reſults, are the very beſt modes of extending the limits of ſcience, and clearing the obſcurity in which many of its branches, and medicine in particular, remain ſtill involved. For, independent of the immediate good effects they may produce, in leading to new and unexpected diſcoveries; unveiling the defects of tenets long and implicitly adopted; and diſſipating, in ſhort, on every ſide, the gloom of ignorance or of error; they are indirectly productive of the moſt deſirable advantages, by promoting a general deſire of knowledge, an emulation to excel in it, a ſpirit of experiment, and a repeated diſcuſſion of ſubjects the moſt intereſting to the

the ſons of ſcience, and the welfare of mankind in general.

Although theſe truths, however, muſt have been always evident to an impartial mind; it will be found, upon contemplating the hiſtory of medicine (and to medicine they apply with greater force, if poſſible, than to any other ſcience), that the greater number of thoſe employed in its purſuit, have ever acted in direct oppoſition to them. Thoſe who have ventured to queſtion opinions once entertained, however ill-founded or abſurd; thoſe who have made public new diſcoveries, or new explanations of facts already diſcovered; inſtead of that candour, moderation, and impartiality, which ſhould ever take place in ſcientific reſearches, have frequently experienced treatment the moſt illiberal and unjuſtifiable.

Various have been the cauſes which operated in producing ſuch effects. Of the numbers employed in the purſuit or practice of medicine, a great part muſt, of courſe, be deficient in the ſtrength and acuteneſs

of genius neceſſary to decide with propriety between a diverſity of opinions: ſuch have always been invincibly prepoſſeſſed in favour of ideas firſt imbibed; confident and petulant in proportion to their deficiencies, they have ever oppoſed improvement, and, ſcreened behind the ſhield of the many, derided the more juſt opinions of the few. Of authors, a number have looked upon their productions with the partiality of parents, and been equally blind to their defects, and the validity of objections urged againſt them: ſuch they have defended, from a ſelf-conviction of their juſtice; but, in the defence, have employed a degree of aſperity, which, though frequently proportioned to the confidence of the author, can never be juſtified in the cauſe they were employed. Others again, though at length convinced that ſyſtems they had once erected or ſupported were totally erroneous; yet, prompted by pride, vanity, or intereſt, have perſiſted in maintaining them, nay, increaſed their exertions againſt the very appearance of innovation.

Such are the caufes, with others which this is not a place to difcufs, to which the partiality for old opinions, the averfion to new ones, and the afperity of difpute, fo frequently remarkable, and fo detrimental to the advancement of medical knowledge, are principally to be attributed ; and though it muft be acknowledged that they ftill continue in fome degree to operate, it is only from a conviction that their effects are much more limited than formerly, that the author of the following fheets would venture to lay them before the public. But freedom of inquiry feems to be the fpirit of the age, and, when conducted with propriety, ever fhould, and will at length, experience the treatment it merits : let its refults be examined with candour and impartiality; and appreciated, not by their degree of variation from great authorities, or opinions generally adopted, but by the facts and arguments adduced in their fupport, and urged againft them.

On the fubject, and manner of conducting the following Inquiry, it may not be

amiſs to offer next a few obſervations. As a thorough acquaintance with the properties of the medicines he employs, is of the utmoſt conſequence to the practiſing phyſician; and as, in our reaſonings with reſpect to the nature of different diſeaſes, concluſions are frequently drawn from the manner in which many remedies employed in their removal are ſuppoſed to affect the body in a healthy or diſeaſed ſtate; ſo a minute inveſtigation into the nature of medicines of noted efficacy cannot be deemed ſuperfluous, but, on the contrary, muſt, if properly conducted, be productive of the beſt effects. Among the many articles daily employed in the practice of medicine, none are more frequently exhibited, none affect the human frame more powerfully, and few are oftener the ſubject of medical reaſoning, than Opium; yet, of all, there is none whoſe nature has been leſs underſtood, or with reſpect to which a greater diverſity of ſentiment is entertained. With this incongruity of opinion the author was forcibly ſtruck, after having been for ſome

time

time engaged in the ſtudy of medicine; and as the ſubject ſeemed deſerving particular attention, he was at pains to compare the ideas of the principal authors on the ſubject, and examine the proofs adduced in their ſupport. Still uncertain which came neareſt the truth, recourſe was had to experiments, which at length led to the concluſions he has attempted to eſtabliſh.

In the firſt chapter of the work is given a brief account of the natural hiſtory of the medicine. The ſecond contains an enumeration of the effects it produces when applied to living ſyſtems, unaccompanied by any ſpeculations as to the mode of their production; as it is principally deſigned as a foundation for our future reaſonings on the ſubject. In the third is given, whatever could be diſcovered with reſpect to the analyſis of its component principles. The fourth chapter contains, a brief account of the different theories which have prevailed with reſpect to its mode of action. The fifth, a refutation of the ideas of thoſe who aſcribe its effects to changes induced in the fluids.

fluids. The ſixth, a diſcuſſion of the remaining opinions on the ſubject, and a detail of that which the author has attempted to eſtabliſh. And in the concluding one are given, practical obſervations on its pharmaceutical treatment, and uſe or abuſe in particular diſeaſes.

In conducting the inquiry, due deference has been paid to the authority of great names; their opinions, when found juſt, have been admitted; when erroneous, arraigned, and the reaſons for their rejection aſſigned. To diſtinguiſh realities from appearances, truth from error, and to explain with perſpicuity the opinions he has adopted, has been the author's principal aim: how far he has ſucceeded, or to what degree theſe opinions are peculiar to himſelf, will be beſt determined by an attentive and impartial peruſal of the ſubſequent pages. In proof of many circumſtances on which his reaſonings are founded, he has been deſirous of adducing, not only his own experience, and experiments, but the joint teſtimonies of other medical authors; hence

a number of quotations are obſervable in the courſe of the work, which have not been collected from any oſtentatious motives, but with a view of corroborating his own aſſertions, and of facilitating the inquiries of ſuch readers as may wiſh to conſult the original writings of the authors alluded to.

Limerick, Auguſt 1792.

An ENQUIRY, &c.

CHAP. I.

Of the Natural Hiſtory of Opium.

IT appears rather ſingular, that almoſt every circumſtance relating to this remarkable medicine has been the ſubject of diſpute; and that authors have materially differed, not only in the more abſtruſe inveſtigation of its component principles and mode of action, but in their accounts of the moſt obvious facts relating to its natural hiſtory. Late experience, however, ſeems ſo far to have aſcertained the manner of its production and preparation, as to remove every difference on theſe points.

Opium is prepared in ſeveral countries, and conſumed in conſiderable quantity, over all the warmer regions of Aſia, and in Egypt and the other parts of Africa where

 the

the Mahometan religion prevails. The Mahometans, indeed, ſeem, in every quarter of the world, much addicted to it, as being denied the uſe of wine and other inebriating liquors by the tenets of their religion. Egypt, and eſpecially the country adjoining Thebes, was long famous for the quantity and excellence of its Opium, and hence the epithet *Thebaic* ſtill given to ſome of its preparations. In all theſe countries where Opium is manufactured, large fields are carefully tilled for the purpoſe of producing it, and its ſale conſtitutes no inconſiderable branch of commerce. The Opium of our ſhops is generally imported from Perſia, Egypt, Smyrna, and other parts of the Levant, in cakes of from four ounces to a pound weight; which are ſometimes covered with the dried leaves of the poppy, but more generally, at leaſt in ſuch ſpecimens as I have ſeen, with the huſks and ſeeds of ſome of the Lapatha or Dock kind; an obſervation long ſince made by Doctor Alſton. The Opium they contain, when cut, appears of a dark brown colour; when reduced to powder, of a yellowiſh brown:

it is of a ſoft tenacious conſiſtence, eſpecially when much handled; its ſmell is peculiar, faint, and diſagreeable; and its taſte bitter, pungent, and acrimonious.

That Opium thus imported is the product of the Papaver, or Poppy, ſeems the only circumſtance in which authors have univerſally agreed. The genus Papaver, which is arranged by Linnæus in the claſs Polyandria, and order Monogynia, is thus ſyſtematically deſcribed *: "*Papaver*. Cor. 4-petala. Cal. 2-phyllus. Capſula 1-locularis, ſub ſtigmate perſiſtente poris dehiſcens." To this genus nine different ſpecies belong; but that from which the Opium is principally obtained is the ſeventh, or "*Papaver ſomniferum* calyce capſuliſque glabris, foliis amplexicaulibus inciſis." It would appear, however, that although this ſpecies is to be preferred, as affording Opium in greater quantity than any of the reſt, yet the other ſpecies ſeem to afford a medicine ſimilar to that obtained from the Pap. ſomniferum,

* Linnæi Syſtema Vegetabilium, p. 407, cura Murray, Gottingæ 1774.

though,

though, from the ſmallneſs of their heads, they muſt yield it in leſs proportion. They all at leaſt, on being wounded, give out a *ſeemingly ſimilar* milky juice; and I have myſelf obtained from the *papaver rhœas* Opium perfectly ſimilar to that got from the *ſomniferum.*

With reſpect to the manner in which the Opium imported into Europe is obtained from the poppy, a diverſity of opinion has been long entertained; ſome aſſerting that it was extracted from the heads, ſtalks, and leaves, by boiling and inſpiſſation; ſome, that it was merely the expreſſed juice inſpiſſated by heat; and others, that it was compoſed of the milky juice obtained by wounding the heads. That, in thoſe countries where Opium is manufactured, an extract is obtained from the poppy plant, by boiling, cannot be denied. Dioſcorides * takes particular notice of it, and diſtinguiſhes it from the juice obtained by wounding the heads of the poppy, which, he ſays, is the true

* De Materia Medica, lib. 4. c. 25.

Opium, by the name *Meconium.* *Pliny* * makes a ſimilar diſtinction, as does Kæmpfer in his Amœnitates Exoticæ, and Bontius in his Medicina Indorum. Many have ſuppoſed that our Opium is merely this extract, and not obtained by wounding the heads; and they ſeem to have been led to this concluſion principally from conſidering, that large quantities are daily conſumed, and that its price is generally moderate. Of this opinion are Proſper Alpinus †, Lemery ‡, Savary §, Condamine ‖, and others. We cannot, however, for various reaſons, accede to it: the only one of its advocates who could determine from actual experience, was Proſper Alpinus; and he probably was led into a miſtake, from the Egyptians adulterating the real Opium with meconium. Kæmpfer, who lived two years in

* Naturalis Hiſtoria, lib. 20. c. 18.

† Medicina Ægyptiorum, l. 4. c. 2.

‡ Dictionnaire des Drogues, art. Opium.

§ Dictionnaire de Commerce.

‖ Mem. de l'Academie des Sciences, pour l' an. 1732, p. 421.

Perſia,

Perſia, aſſerts that it is obtained from the heads by inciſion, and particularly deſcribes the operation. The inciſions, he ſays, are made with a five-edged knife, in the evening, and the juice being collected next day, is inſpiſſated to the conſiſtence of Opium *: a ſimilar account is given by Garcias †. But that our Opium is obtained by inciſion, is placed beyond a doubt by Mr. Ker, who has given a very accurate deſcription of the manner in which the poppy is cultivated, and the Opium obtained from it by inciſion, in the province of Bahar in the Eaſt Indies. The ſeeds, according to him, are ſown in quadrangular areas, the intervals of which are formed into aqueducts for conveying water to each area. The plants are allowed to grow ſix or eight inches from each other, and are plentifully ſupplied with water till they are ſix or eight inches high, when a nutrient compoſt of dung, aſhes, and nitrous earth, is laid over

* Amœnitates Exoticæ, Faſc. 3. Obſ. 15.
† Hiſtoria Aromatum & Simplicium, lib. 1. c. 4.

the areas. A little before the flowers appear they are again well watered, till the capſules are half grown, when the watering is ſtopped, and they begin to collect the Opium. This they effect by making at ſun-ſet two longitudinal inciſions from below upwards, without penetrating the cavity, with an inſtrument which has two points as fine and ſharp as a lancet; the inciſions are repeated every evening until each capſule has received ſix or eight wounds, and they are then allowed to ripen their ſeeds. The juice which exudes is collected in the morning, and, being inſpiſſated to a proper conſiſtence, by working it in an earthen pot in the ſun's heat, is formed into cakes for ſale *.

Beſides this very particular account given by an ocular witneſs, of the manner in which Opium is extracted by inciſion in the Eaſt Indies; we have further proof that all

* Med. Obſervations and Inquiries, vol. v. art. 28. See alſo, in ſupport of the ſame opinion, Chardin's Travels into Perſia.

produced in Perſia, Natolia, and the other countries which ſupply us, is obtained in a ſimilar way, from conſidering that Opium may be extracted by inciſion from the poppies of our own climate, perfectly ſimilar to that imported from theſe countries; while neither the extract of the poppy procured by boiling, nor the inſpiſſated expreſſed juice, bear any reſemblance to it. Theſe facts ſeem to have been firſt aſcertained by Dr. Alſton *. I have myſelf extracted from our own poppies a pretty conſiderable quantity of Opium, which differed from that of the ſhops only in ſmelling ſtronger, and being to the taſte more bitter and pungent; its ſuperiority, however, in theſe reſpects, ſeemed gradually to diminiſh. In obtaining it, I followed ſometimes the method mentioned by Kæmpfer, making five inciſions at a time; ſometimes that deſcribed by Mr. Ker, making but two; and think the one anſwers in the end juſt as well as the other. The dark colour of the

* Med. Eſſays, vol. v.

Opium, as I found during theſe experiments, is in ſome meaſure, though not entirely, owing to the iron inſtruments employed in collecting it; the chalybeate ſtriking a dirty black colour with the aſtringent matter of the juice: for when I employed a ſharp piece of glaſs in making the inciſions, and a ſhell in collecting the juice, I found the Opium produced of a clearer reddiſh brown than that uſually obſervable.

But although it be certain that pure Opium is procured by inciſion alone, it cannot be denied that it is frequently, nay generally adulterated, and mixed with the expreſſed juice, extract of the plant, and other foreign ſubſtances. Mr. Ker ſays it is frequently mixed with cow-dung, the extract obtained by boiling, and other additions which are kept ſecret. Nevertheleſs, that this adulteration is not very conſiderable in the Opium imported into theſe kingdoms, appears ſufficiently from the following experiment.

In twelve parts of Opium officinarum, there will be generally found from three to

three and a half at moſt, of feculencies indiſſoluble in water or ſpirit of wine; and in twelve parts of Opium which I myſelf collected, and to which no extraneous ſubſtance was added, I found two indiſſoluble in any menſtruum; which ſeemed principally compoſed of the external cuticle of the capſule, which was ſeparated in ſcraping off the juice; ſo that in the common Opium of the ſhops, *one part and a half* out of *twelve*, ſeems to be the *full* amount of any extraneous addition. Mr. Ker ſuppoſes that the poppy may be cultivated to advantage, on grounds of little value, in our own climate. An acre, he ſays, yields in the Eaſt Indies ſixty pounds of Opium, which, at nine ſhillings per pound, is 27*l*. an acre. Whether in this country it could be obtained in equal quantity, I cannot pretend to determine; the ſpot which produced the plants from which I extracted the Opium, being a rich garden mould, and ſo ſmall that it would be wrong to hazard a concluſion on this head: the experiment, however, may be eaſily made, and ſeems worthy of attention.

attention. If any overplus remained after our own demands, a ready market would be found for it in the Eaſt Indies, where its conſumption is very conſiderable, and price generally high.

CHAP. II.

An Account of the Effects of Opium on living Systems.

ON ſome parts of the ſubject of the preſent chapter, we find as great a diverſity of opinion as in the former. The truth, however, where diſputed, I have endeavoured to aſcertain by experiments, which have at leaſt the merit of being faithfully related; and ſuch are the teſts to which, in all ſimilar caſes, we muſt ultimately appeal. I ſhall here barely deſcribe the conſequences reſulting from the operation of Opium on living bodies, deferring the explanation of the manner in which they are produced, to a future part of the work. And as an accurate and comprehenſive view of its effects ſhould be the chief foundation for our reaſonings with

refpect to its mode of operation; as thefe effects can be more clearly afcertained in a ftate of health, than when complicated with the fymptoms of difeafe; and as its operation in various morbid affections will be more properly confidered after its mode of action has undergone examination, I fhall here chiefly confine myfelf to an enumeration of its effects on the body in a ftate of health, beginning with thofe refulting from its external application.

Opium applied to the eye, internal membrane of the nofe, urethra, or other fimilar fenfible furface, or to any part of the body deprived of its cuticle, is firft productive of pain, a fenfe of heat, and of inflammation; after the ceffation of which fymptoms the natural or morbid fenfibility of the part is diminifhed.

That Opium diminifhes the fenfibility of parts to which it is applied, has been generally acknowledged; but that this diminution of fenfibility is preceded by the effects above ftated, has been difputed by many;

ſome entirely denying their production, and others deeming them ſo inconſiderable as ſcarcely to merit attention. The following experiments however lead to a very different concluſion, and ſeem ſufficiently to eſtabliſh the facts above laid down.

Experiment I.

I poured a ſmall quantity of a ſtrong watery ſolution of Opium * into my left eye; it immediately occaſioned conſiderable pain, which continued for about ten minutes, and a ſmart degree of inflammation both in the tunica adnata and palpebræ, which remained very obſervable for many minutes after the pain had ceaſed; the pain and inflammation were likewiſe attended with an effuſion of tears. After thirty minutes had elapſed from the commencement of the experiment, I poured into both eyes a ſmall quantity of ſpirits di-

* This ſolution, and that uſed in all the ſucceeding experiments, was made by diſſolving Opium in warm water, in the proportion of one drachm of the former to two ounces of the latter.

luted

luted with water; the pain they occaſioned was more conſiderable in the right eye than in the left.

Experiment II.

Having reduced a ſmall quantity of Opium to powder, I drew it up my noſe: it produced the ſame effects as weak ſnuff would have done. This experiment was made with ſimilar event by Dr. Alſton: Opium, he ſays, heats and irritates the noſe, and creates an inclination to ſneeze *.

Experiment III.

I injected ſome of the ſame ſolution above mentioned into the urethra, which was immediately followed by a ſenſe of heat and pain. The reſult of this experiment I have indeed ſeen verified in ſeveral inſtances, wherein a ſimple aqueous ſolution of Opium was uſed by way of injection in gonorrhœa; for although ſuch application will, after ſome time has elapſed, allay

* Edinburgh Medical Eſſays, vol. v. art. 12. ſect. 3.

perhaps

perhaps the pain and irritation, its primary effect, as I have uniformly obferved, is to increafe them.

EXPERIMENT IV.

Having fcraped off the cuticle above my wrift, I applied to the part fome of the aqueous folution of Opium, which manifeftly increafed the pain of it. This fact is likewife corroborated by Dr. Alfton. I have feveral times, fays he, applied an aqueous folution of Opium to parts excoriated, and fuperficial wounds, and always found it hot and irritating like weak fpirits; the pain continuing for fome minutes *.

But although the effects juft ftated evidently refult from the external application of Opium to the parts of the body abovementioned; yet on others endowed with a lefs degree of fenfibility, and covered with the common cutis and cuticula, it feems to exert no powers whatever. Bergius, indeed, in his Materia Medica, afcribes con-

* In loc. citat.

ſiderable effects to it when applied to the ſkin—" Opium," ſays he, " externe cuti applicatum, agit ſtimulando in ſimul ſopiendo; ſi diutius ſupra cutem manet, rubidinem vel veſiculas excitat, ubi nimirum cutis tenera eſt opiumque recens *." It has beſides been a generally received opinion, that Opium, externally applied, diminiſhes ſenſibility, allays pain, and is attended, in ſhort, with all the other effects reſulting from its internal exhibition. Hence, in all diſpenſatories we find a variety of anodyne balſams, liniments, and plaiſters, deſigned for external application. But whether the efficacy of all ſuch compoſitions, if any they poſſeſs, depends upon the Opium they contain, may to ſome appear at leaſt doubtful from the following

Experiment V.

I applied, at the ſame time, round one of the fingers of my right hand, and round my left arm about midway between the

* Materia Medica, tom. ii.

wriſt

wrift and elbow, a large portion of Opium, and fuffered it to remain there eighteen hours; it was productive of no peculiar fenfation, and the parts to which it was applied differed in no refpect, after its removal, from thofe which furrounded them. This experiment was alfo made, and with like event, by Dr. Alfton *. Wedelius likewife fays, " Nos nunquam ftuporem partis ab impofitione opiatorum obfervare potuimus †." Felix Platerus deduced the fame conclufions from his experiments ‡; and the celebrated French furgeon, Louis ||, confirms the fact from his own experience and that of many others. The authority however of a late moft refpectable character, Dr. Cullen, whofe practical remarks fhould ever be looked up to, would lead us to conclude, that fome effects refult from the application of Opium to the common integu-

* Medical Effays, vol. v. art. 12. fect. 3.

† Opiologia, lib. ii. fect. 3. cap. 1.

‡ Praxis Medica, lib. i. cap. 5.

|| Recueil des pieces qui ont concurru pour le prix de l'Acad. Royal. de Chirurg.

ments.

ments. He ſays, he has certainly found them, thus employed, relieve the pains and ſpaſms of the ſtomach and inteſtines *. Whether the facts he mentions were to be deemed concluſive, in conſequence of having truſted the removal of ſuch complaints to external applications *alone*, or otherwiſe, we are not informed; nor can I, from my own experience, offer any thing deciſive on this point, having in ſuch caſes never neglected internal remedies; though I think I have found them aſſiſted by the external application of opiates. Lind ſays, that in a caſe of Tetanus, an application of Opium and camphire to the feet, inſtantly removed the ſpaſm; which, upon taking off the application, immediately returned with its former violence; an effect which was ſeveral times produced by the repeated application of theſe medicines. Moſely, on the contrary, who quotes this paſſage, aſſerts from his own experience, that opiates externally applied are not of the ſmalleſt utility, either

* Materia Medica, vol. ii. p. 256.

in the prevention or cure of a Tetanus *. Whytt, in cafes of great pain and ficknefs of ftomach, where the patient could not bear laudanum internally, experienced the beft effects from its external application to the belly and region of the ftomach †. On mentioning the matter to my friend Dr. Harding, he tells me he has feen two very pointed and decifive inftances of the efficacy of the Tinct. Theb. rubbed to the region of theftomach, in removing obftinate vomiting, and allaying the painful and fpafmodic affections to which that organ is liable.

The effects of Opium taken internally, come next to be examined; and as their great variety requires fome arrangement, I fhall confider it as affecting—1ft, the vital—2d, the natural—and 3d, the animal functions.

From an adequate dofe of Opium, the following changes are obfervable in the vital functions.—The pulfations of the heart

* On Tropical Difeafes, p. 494.

† On Nervous Diforders, c. vii. p. 362.

and arteries are firſt rendered quicker, fuller, and ſtronger, and afterwards ſlower than at the time of taking it. With the increaſe of frequency in the pulſe, the heat of the body is generally ſomewhat augmented; the reſpiration is little affected, except a large doſe has been taken; towards the concluſion of the operation of which it becomes ſlow, ſtertorous, and laborious.

Diametrically oppoſite have been the opinions of phyſicians, with reſpect to the changes induced by Opium on the pulſe; many poſitively aſſerting that its only effect is to render them ſlower and weaker; while even a greater number, of equal authority, unite in aſſerting, that although this be the ultimate reſult of its action, the primary effect of its operation is to render them quicker and ſtronger. Boerhaave, ſpeaking of cordial remedies, ſays, "Huc etiam pertinent Opium et alia *circulum augentia* *." Friend aſſigns it the ſame property †. Dr. Cullen, in his Materia Medica, aſſerts, that

* Tractatus de viribus remediorum.

† Emmenologia, cap. xiv.

"Opium,

" Opium, in its firſt operation, irritates the ſanguiferous ſyſtem, and excites the force of the circulation *." And Tralles, in his voluminous treatiſe on this medicine, aſſerts its power of quickening and raiſing the pulſe, in ſuch ſtriking terms that I cannot help copying them : " Denuo itaque," ſays he, " repeto & ad experientiam audacter provoco, prima ſua actione ſemper Opium pulſum accelerare ; quanquam vel finita ejus operatione, vel aucta ultra modum doſi quæ necem adferre poſſit, omnino eveniat ut ex celeri, is tardus admodum et debilis reddatur †." The author here boldly appeals to experiment for the truth of what he advances. Willing to aſcertain, if poſſible, the fact in ſo important a point, I had recourſe to the ſame teſt, and made the following trials, with all imaginable care and exactneſs.—The room in which they were conducted was of a moderate temperature ; all motion was as much as poſſible refrained from during the whole experiment ; and

* Vol. ii. p. 221, 229, & alibi.

† Uſus Opii, vol. i. p. 64.

every

every circumſtance in ſhort avoided, that could give riſe to miſtake. The experiments were ſeveral times repeated, but with ſuch trifling variations as to the main point to be determined, that a particular detail of the three firſt only, ſeems fully ſufficient.

Experiment VI.

Having breakfaſted lightly at nine in the morning, I took at twelve one grain of Opium diffuſed in a tea-ſpoonful of warm water—my pulſe beat 70 in a minute, their natural ſtandard; and the following were the changes obſerved in them: They were reckoned immediately before, at, and after the periods mentioned in the firſt line of the table; and if any material difference occurred, which was very ſeldom the caſe, a medium was ſtruck between the three, and inſerted as the number of pulſations at the time ſpecified.

In	2	5	10	15	20	25	30	35	40	45	50	55	60	minutes,
P. beat	70	74	76	76	74	74	74	72	72	70	70	70	70	

I could ſcarcely perceive any variation in the ſtrength or fulneſs of the pulſe during the whole experiment; nor was it ſucceeded by drowſineſs, or any ſimilar affection.

Experiment VII.

At one o'clock, P. M. I gave to a robuſt healthy young man, whoſe pulſe beat but 44 in a minute, its natural ſtandard *, one grain of Opium diffuſed in a ſmall quantity of warm water. He had never before taken any of the medicine, and his pulſe was affected in the following manner:

In	5	10	15	20	25	30	35	40	45	50	55	60	70	80	90	100	110	120	135	min.
P. beat	44	44	44	44	50	52	54	48	48	46	46	46	46	44	42	42	40	40	44	

After 25 minutes had elapſed, there was a manifeſt increaſe as well in the ſtrength and fulneſs, as in the frequency of the pulſe. In an hour this began to diminiſh, and continued decreaſing till near the end of the experiment. A ſlight heavineſs, which

* I repeatedly examined his pulſe at different periods afterwards, and always found them beat but from 42 to 46 in a minute.

came

came on 55 minutes after he had taken the Opium, was the only other effect experienced from it.

Experiment VIII.

Forty-five minutes after twelve, P. M. my pulſe beating 70 in a minute, I took two grains and a half of Opium diſſolved in an ounce of water:

In	5	10	15	20	25	30	35	40	45	50	55	60	75	90	minutes,
P. beat	74	74	74	76	78	80	72	70	64	64	66	70	70	70	

In twenty minutes perceived a ſlight warmth, and ſoon after a degree of moiſture on my ſkin, the fulneſs of pulſe increaſing as well as its frequency. In half an hour I found myſelf, or at leaſt imagined myſelf, more alert and ſprightly than before; in 40 minutes perceived a pleaſing kind of languor gradually increaſing; in 90 minutes a dull head-ach; in two hours time the head-ach was much increaſed, and attended with drowſineſs and nauſea; in two hours and a half every diſagreeable ſymptom was increaſed, my pulſe 70; took a ſpoonful of

 vinegar,

vinegar, which ſomewhat relieved the nauſea ; in two hours and three quarters found all the above ſymptoms ſtill increaſing, and attended with ſlight vertigo, and tremors in my hands—pulſe the ſame as before. In three hours and a half the nauſea was conſiderably augmented, the other ſymptoms as before, and I at length threw up the contents of my ſtomach. The head-ach and vertigo were ſoon after relieved ; but I continued in a ſtupid ſtate for the remainder of the day.

Theſe experiments, and the authorities above quoted, ſeem ſufficiently to evince, that the primary effect of Opium on the pulſe is to accelerate and render it fuller ; and we can only account for the miſtake of thoſe who maintain a contrary opinion, by ſuppoſing that they neglected to examine the ſtate of the pulſe *ſhortly* after the Opium had been taken, attending only to the *ultimate* changes it underwent ; and this ſuppoſition will receive further confirmation from conſidering, that the only favourer of this opinion, who, as far as I know, has given

given a particular detail of any experiments in its ſupport, has totally omitted any account of the ſtate of the pulſe during the firſt half hour *; although, as ſufficiently appears from thoſe above related, its frequency is, during that period, more augmented than during any other, and indeed in a ſhorter time than has been generally imagined. After this increaſed frequency of pulſe has continued for ſome time, it again becomes ſlower; and this change, as may be obſerved in the eighth experiment above ſtated, and as I have remarked in many others of a ſimilar nature, is frequently *very ſudden.* The number of pulſations, in a given time after this change has once taken place, continue to diminiſh, and at length, if the doſe has been any way conſiderable, fall far ſhort of the natural ſtandard of health; the difference in this reſpect being generally proportioned to the quantity of the medicine which has been taken, and the conſequent increaſe of quick-

* Bard. Diſſertatio Inauguralis de Opio, Edinburgi edit. an. 1763.

neſs occaſioned by it in the heart's pulſations. If the doſe has been ſo great as to induce death, intermiſſions in the pulſe have been generally remarked before that event took place.

With the increaſed frequency of pulſe, the heat of the body is, as above mentioned, ſomewhat augmented; at leaſt in all my experiments I found this to be the caſe, if I could judge by my own feelings, and it was ſometimes attended with fluſhings of the face. I could perceive little alteration in the reſpiration, except the doſe had been conſiderable; by ſuch it was in the end rendered ſomewhat laborious. Thoſe who have had an opportunity of remarking the ſymptoms which preceded death from an over-doſe, have obſerved it ſlow, ſtertorous, and laborious.

The natural functions are affected by Opium in the following manner:

The appetite and digeſtion, from unuſually large, or frequently repeated doſes, are generally impaired, and vomiting often induced; the diſcharges from the inteſtines

are diminiſhed or ſuppreſſed; ſecretion and excretion are impeded in every part of the ſyſtem, except the ſkin, the diſcharge from which is evidently augmented.

Theſe effects are generally known and acknowledged to reſult from the operation of Opium. The eaſtern nations are ſo well convinced, by experience, of its powers in diminiſhing the appetite, that, in the famine which prevailed in the Eaſt Indies in the year 1770, the wretched ſufferers purchaſed it at exorbitant prices, to allay the cravings of hunger, and ſmooth the approach to death *. That its too free uſe impairs digeſtion, and induces vomiting, is well known; but all theſe effects would ſeem only to ariſe from an over-doſe of the medicine, or from its repeated and conſtant exhibition. Its powers in leſſening the inteſtinal evacuation need no proof. That it diminiſhes the ſecretion of the mucus which moiſtens the mouth and fauces, is evident from the thirſt it is productive of; and that

* Kerr. Lond. Med. Obſerv. vol. v. art. 28.

the ſecretion, or, more probably, the excretion of other fluids are impeded by it, is at leaſt probable; the gall and urinary receptacles having been found in thoſe animals, to whom conſiderable doſes were given, replete with their reſpective fluids *; and the generality of writers agreeing in pointing out theſe as among its moſt conſtant effects.

Some authors have indeed deemed it capable of increaſing the urinary ſecretion, and hence recommend it in the cure of dropſy. Bartholine † and Willis ‡ long ſince adviſed its exhibition in aſcites, aſſerting, it has ſometimes proved uſeful in that diſorder, by increaſing the renal, as well as cuticular diſcharge. Dr. D. Monro has likewiſe deemed it, in ſome inſtances, ſerviceable; and, in further ſupport of the

* Spræegel Experiment. circa Venena, apud Haller, Diſſert. Pract. vol. vi. Exp. 15. & ſequent.——Haller de Corp. Human. Irritabilitate, in Comment. Gottingenſ. ——Kann. Boerhaave, Impet. faciens, p. 402, 403.

† Medicinal. Epiſtolar. Centuriæ quatuor, cent. iii. epiſt. 46 & 49.

‡ Pharmacop. Ration. p. i. ſ. 7. c. 1.

opinion,

opinion, two cafes of the difeafe have lately been given to the public, wherein it was fuppofed to have performed a cure *. In one cafe, half a drachm of the medicine was infufed in half a pint of gin, of which the patient took a tea-fpoonful once or twice a day, and efcaped the dangers of the diforder; we can fcarcely, however, accede to the conclufion drawn from this inftance; for, on confidering the compofition of the medicine employed, it will be found, that the patient did not take above a grain of Opium in four-and-twenty hours; but how often has it been given in fimilar or more confiderable dofes, without being obferved to increafe the quantity of urine; and how frequently have the powers of nature removed, unaffifted, the ftagnant fluids of dropfical patients †. The other cafe contained

* Lond. Med. Obfervations, vol. vi. art. 3.

† Of this I have myfelf feen one very remarkable inftance. The patient laboured under the various fymptoms of univerfal dropfy, and was taking no medicines, when the quantity of his urine became vifibly increafed, amounting to fix, eight, nay ten pints a day; and

tained in the ſame paper is ſtill leſs in point, ſeveral other medicines having been employed at the ſame time with the Opiate. Perhaps in the thoſe caſes wherein the medicine appeared to act as a diuretic, it operated by removing ſome ſpaſmodic contraction of the renal veſſels; the inſtances are, however, ſo few, and of ſuch nature, that we ſhould be at leaſt allowed to doubt its diuretic powers, till further confirmed by more extenſive experience *.

But though Opium generally diminiſhes all the other diſcharges, it certainly augments that from the ſkin; this effect has been univerſally noticed, and it is found

and this increaſed diſcharge continued, till the ſwelling and other ſymptoms entirely diſappeared, which was effected in the courſe of 15 days.

* Since writing the above, I have, for the firſt time, ſeen a paper, by Dr. Michaelis, in the firſt vol. of the Medical Communications, on the effects of Opium in the venereal diſeaſe; he gave it in uncommonly large doſes, and found it in many caſes produce a plentiful ſecretion of urine, exceeding in quantity all the fluids drank. In ſome theſe large doſes produced an increaſed ſecretion of ſaliva, in others a diarrhæa.

peculiarly

peculiarly efficacious as a ſudorific, in many diſeaſed ſtates of the ſyſtem. The increaſe of perſpiration I have generally obſerved ſoon to ſucceed the encreaſe of frequency in the pulſe, as is particularly noticed in Experiment VIII. but in place of decreaſing as the pulſe lowered, I have found it for the moſt part continue after the frequency of the pulſe was reduced below the natural ſtandard. The increaſe of perſpiration from Opium, is likewiſe, in ſome conſtitutions, preceded by, or attended with, a ſenſe of pricking, or itching of the ſkin. In ſome an eruption ſimilar to the miliary has been obſerved to accompany it, to diſappear on omitting the Opium, and again break forth on renewing its exhibition *.

In the animal functions, the principal alterations occaſioned by Opium are the following:

* Tralles, vol. i. p. 138.——Lond. Med. Obſervations, vol. iv.——Friend, Emenolog. cap. 14. whoſe enumeration of the effects of Opium is very juſt and exact, though his explanation of its mode of operation be purely theoretical.

The hilarity of mind is by degrees augmented, and continues to increaſe, if the doſe has been conſiderable, until the delirium of intoxication is produced; which, as when reſulting from ſpirituous liquors, is attended in different conſtitutions with different ſymptoms. It is, however, more generally productive of a pleaſant and joyous ſtate of mind than the contrary; and it, in many, occaſions an increaſed diſpoſition to venery. After theſe effects have continued for ſome time, they are ſucceeded by others of a very oppoſite nature; the mind becomes gradually dull and languid, the body averſe to motion, little affected by cuſtomary impreſſions, and inclined to ſleep. If the doſe has been conſiderable, all theſe ſymptoms continue to increaſe; and tremors, convulſions, vertigo, ſtupor, inſenſibility, and deprivation of muſcular action appear variouſly complicated, and in various degrees, proportioned to the exceſs in the doſe, and peculiarity of conſtitution in the ſufferer.

In ſupport of theſe facts little need, I apprehend, be advanced; but as it has been

diſputed

disputed whether some of them follow, either generally or to any considerable degree the exhibition of Opium, I shall state such experiments, facts, and authorities, as appear convincing enough to remove every doubt.

With respect to the ultimate effects resulting from an over-dose of Opium, all writers seem to agree, they are those above stated, as will appear from recurring to every history of such unfortunate sufferers as have, either through mistake or design, taken it in uncommon large quantities. Doubts have, however, been started with respect to its previous enlivening and exhilarating powers, especially on the inhabitants of these countries, and others not so much accustomed to its use as the eastern and Mahometan nations.

To determine this point, I shall not here adduce any particular experiment, but merely observe that I have myself, frequently and uniformly, experienced from large doses an increased flow of spirits, an observablegaiety, cheerfulness, and alertness, which, subsiding

into

into a ſtate of pleaſing languor, terminated ultimately in a degree of drowſineſs, ſtupor, and diſinclination to motion. The ſame effects I have had ſeveral opportunities of obſerving in others, to whom, in the courſe of my experiments, I have given the medicine in doſes above the uſual ſtandard.

Some, when queſtioned with reſpect to the manner in which they felt affected, have frequently compared its operation and effects to thoſe ariſing from ſpirituous liquors. A very ſatisfactory experiment on the preſent ſubject of conſideration is given us by Doctor Leigh *; it was made by Doctor Ramſay one of his friends, and is related as follows, in his own words:

" Finding myſelf one night at eleven more diſpoſed to ſleep than uſual, I determined to try the effects of opium, and took immediately thirty drops of the Theb. Tinct. The doſe ſoon began to operate, and produced ſuch enlivening effects as enabled me to proſecute the ſtudy in which I was then

* Experimental Inquiry into the Properties of Opium, p. 113.

engaged.

engaged. In this cheerful ſituation I remained till one o'clock in the morning, when I found a violent drowſineſs coming on, which in a ſhort ſpace of time increaſed to ſuch a degree as to render it difficult for me to avoid falling aſleep. I then took between ninety and one hundred drops of the ſame Theb. Tinct. which ſoon rouſed me from my drowſineſs, and invited me once more to engage in my buſineſs. This diſpoſition continued but a ſhort time: I ſoon found myſelf ſo exhilarated, as to grow careleſs of my occupation, and rather inclined to indulge in an exceſs of gaiety; which was gratified for ſome time by ridiculous exceſſes of dancing, ſinging, &c. The powers of my mind ſtill remained ſo perfect, as to enable me to attend to my conduct, and to examine the ſtate of my pulſe, which was ſtrong and full; but not having a proper watch, could not aſcertain its frequency. Thoſe ſymptoms ſoon increaſed to ſo violent a degree as to alarm me; the pulſations of the temporal arteries became uncommonly ſtrong, and every ob-

ject appeared multiplied and covered with a mist."

The remaining part of the experiment it seems here unnecessary to detail particularly; the usual consequences of inability of motion, disturbed sleep, loss of appetite, and at length convulsions followed.

In the eastern countries, where Opium is taken in very large quantities, its enlivening and exhilarating effects are universally known and acknowledged. The Turks, and other nations, swallow it in large quantities, when marching to battle, or under any other circumstances, which require a mind void of depressing fears, and inspired with fortitude. If oppressed by cares or misfortunes, they have recourse to the same assistance, and from its exhilarating powers experience a temporary suspension of melancholy and anxiety. In short, like wine and spirituous liquors in civilized Europe, it is in these countries the support of the coward, the solace of the wretched, and the daily source of intoxication to the debauchee. Of the crowd of eastern travellers, who all

agree

agree in noticing theſe effects of Opium, I ſhall content myſelf with quoting three; the learned Ruſſell, the accurate Chardin, and a late traveller, the Baron de Tott.

Doctor Ruſſell *, after remarking, that the uſe of Opium is not ſo common among the Turks as is generally imagined, being chiefly confined to debauchees, ſays, "They who take it in large quantities are called *Tereakys*, from the term Theriaca Andromachi. At preſent they not only uſe it in that form, but have various other electuaries or confections, wherein it is mixed with aromatics. Some few uſe it pure, and the greateſt quantity I ever knew taken was three drachms in twenty-four hours. The immediate effect I obſerved it to have upon ſuch as were addicted to its uſe, was, that their ſpirits were exhilarated, and from a dozing, depreſſed ſtate, they became active and alert."

Chardin obſerves †, that the Perſians have ſeveral preparations of the poppy; "the

* Hiſtory of Aleppo.

† Travels through Perſia.

firſt is the juice of the poppy itſelf, which they uſe in form of a pill, of the bigneſs of a pin's head at firſt, and then gradually increaſe it to the ſize of a pea; in one hour's time they begin to feel its effects. The Perſians ſay it entertains their fancies with pleaſant viſions, and a kind of rapture; they very ſoon grow merry, then burſt into a laugh, which continues till they die away in a ſwoon. It is found by thoſe who have a diſpoſition for jeſting, to encreaſe that extremely. After the operation of this remedy, the body grows cold, penſive, and heavy; and in this dull and indolent ſituation it remains till the doſe is repeated. When a Perſian finds himſelf in a diſtreſſed ſituation he has recourſe to a piece of Opium as big as his thumb, and immediately after taking this he drinks a glaſs of vinegar; this throws him into a fit of laughter, and every extravagancy of mirth, which terminates in death.——There is a decoction which is made of the ſhells and ſeeds of the poppy; this the Perſians call *Locquenor*; they ſell it publicly in all their cities, as they do coffee: it

it is curious to obſerve the countenances of thoſe who uſe this decoction, before its operation and when its effects have taken place. When they come into the decoction-houſe they are dull, pale, and languid; but as ſoon as the remedy begins to operate, they are quite changed, they run into all the extravagancies of mirth and laughter; and ſuch an uproar is produced, that it would be more proper to give it the name of the mad-houſe, than decoction-ſhop."

The Baron de Tott is very particular in his account of the effects of Opium on the Turks. Speaking of thoſe who give themſelves up to its immoderate uſe, he ſays, "Deſtined to live agreeably only when in a ſort of drunkenneſs, theſe men preſent above all a curious ſpectacle, when they are aſſembled in a part of Conſtantinople called *Teriaky Tcharchiſſy*, The Market of Opium-eaters. It is there that, towards evening, one ſees the lovers of Opium arrive by the different ſtreets which terminate at the Solymania, whoſe pale and melancholy countenances would inſpire only compaſſion;

 did

did not their ſtretched necks, their heads twiſted to the right or left, their back bones crooked, one ſhoulder up to the ears, and a number of other whimſical attitudes, which are the conſequence of the diſorder, preſent the moſt ludicrous and the moſt laughable picture. A long row of little ſhops is built againſt one of the walls of the place where the moſque ſtands. Theſe ſhops are ſhaded by an arbour, which communicates from one to the other; and under which every merchant takes care to place a ſmall ſopha for his cuſtomers to ſit on, without hindering the paſſage, who place themſelves in ſucceſſion to receive a doſe proportioned to the degree of habit and want they have contracted. The pills are ſoon diſtributed; the moſt experienced ſwallow four of theſe, larger than olives; and every one drinking a large glaſs of cold water upon it, waits in ſome particular attitude for an agreeable reverie, which, at the end of three quarters of an hour, or an hour at moſt, never fails to animate theſe machines, and make them geſticulate in a hundred different manners;

but

but they are always very extraordinary and very gay. This is the moment when the ſcene becomes moſt intereſting, all the actors are happy; each of them returns home in a ſtate of total ebriety, but in the full and perfect poſſeſſion of an happineſs which reaſon is not able to procure him. Deaf to the hootings of the paſſengers they meet with, who divert themſelves by making them talk nonſenſe, every one of them firmly believes himſelf in poſſeſſion of what he wiſhes; they have the appearance and the feeling of it; the reality frequently does not produce ſo much pleaſure. The ſame thing happens in private houſes, where the maſter ſets the example of this ſtrange debauch. The men of the law are the moſt ſubject to it, and all the Derviſes uſed to get drunk with Opium before they learned to prefer the exceſs of wine *."

* Memoirs of the Baron de Tott on the Turks and Tartars, vol. i. p. 160. In another place he ſays, "My language maſter, of Perſian extraction, a great enthuſiaſt in poetry, uſed to get drunk indifferently with Opium or with brandy." Ibid. p. 43.

After theſe experiments upon the effects of Opium on the human ſyſtem in a healthy ſtate, many were made on animals; but a few of which I ſhall here recapitulate. The greater number will be more properly introduced in ſome ſubſequent chapters, being connected with the diſcuſſions therein contained.

Experiment IX.

I forced a ſmall quantity of my aqueous ſolution into the eye of a dog; it was productive of pain, a very ſmart degree of inflammation, and an effuſion of tears, ſimilar to thoſe noticed in Experiment I. but more conſiderable.

Experiment X *.

Through a very ſmall opening I injected into the cavity of the abdomen of a rabbit

* The apparent inhumanity of this and many ſucceeding experiments upon animals, will, I hope, be ſufficiently excuſed by the neceſſity which frequently occurs of reſorting to them in our enquiries concerning many points among the moſt intereſting to mankind.

about

about half an ounce of the ſame aqueous ſolution; preventing as much as poſſible the admiſſion of air. Twenty minutes after the animal was opened, and the ſurface of the inteſtines appeared much more red than uſual, and apparently inflamed.

EXPERIMENT XI.

I raiſed the ſternum and laid bare the contents of the thorax of a dog which had been juſt hanged; the heart was ſtill contracting, but in a very irregular manner. I opened the pericardium and waited till all contraction ceaſed, and then let fall on it a few drops of the aqueous ſolution, milk warm. The contractions were inſtantly renewed, and continued for about a minute, and the ſurface of the heart became unuſually red. Upon the contractions ceaſing, they were again renewed, but for a ſhort time, by a ſimilar application: a third attempt was ineffectual; but on injecting a ſmall quantity into the ſuperior venous ſinus, the whole heart was again thrown into action, which continued for a ſhort time, and was

the laſt effort it could be rouſed to. I made a ſimilar experiment with plain warm water. Its firſt application produced a ſlight renewal of the heart's contractions, the ſecond and third were ineffectual.

EXPERIMENT XII.

Having ſeparated by inflation the ſkin and muſcles of the lower extremities of a frog, I poured between them a quantity of the watery ſolution; both limbs were deprived of the power of motion in about nine minutes; the animal died in fifty-five minutes.

CHAP.

CHAP. III.

Of the Analysis of Opium, and the Effects of its different component Principles.

IN examining the component principles of Opium, I have entirely omitted any analysis by fire. A little consideration will sufficiently prove, that the productions thus obtained bear not the most distant resemblance to the principles naturally composing vegetable substances. Many the most dissimilar in nature are in this way resolved into principles exactly alike, and which had no existence in the original subject of investigation. Such experiments, indeed, though once much attended to, are at present universally neglected; even Geoffroy himself, after all his labours, was in the end convinced of their total inutility.

Another

Another mode of inveſtigating the component principles of vegetable productions, has been more generally and juſtly adopted, namely, diſtillation with water, and the application of different menſtrua, adapted to the ſeveral principles the ſubject of inquiry may contain. This method I have purſued, and ſhall lay the reſult thereof in a ſummary manner before the reader.

My firſt endeavours were to determine the relative proportion of gummy, reſinous, and indiſſoluble matter the different ſpecimens of Opium which I examined contained. I, for this purpoſe, followed the uſual plan of applying diſtilled water and pure rectified ſpirit of wine to different portions of Opium, and ſubjecting the reſiduum of each menſtruum to the action of its oppoſite, that of water to ſpirit of wine, and that of ſpirit of wine to water. By weighing exactly both menſtrua previous to their action on the medicine, and after they had extracted all that was poſſible, and calculating the difference, I endeavoured to approach preciſion as nearly as poſſible,

ble, and think Dr. Alſton's concluſion on this point comes nearer to the truth than any I have ſeen. *Twelve* parts of Opium officinarum, he ſays, contain *five* parts of gum, *four* of reſin, and *three* of terreſtrial fœculencies *. My experiments lead me to conclude, that the gummy and reſinous matter are preſent in nearly equal proportion †. With reſpect to the quantity of indiſſoluble fœculencies, we perfectly agree. I very uniformly found them amount to three parts out of twelve; very rarely to three and a half: and although I examined ſeveral different ſpecimens, their ſimilarity in this particular was very ſtriking. I deem it unneceſſary, I am ſure it would be found tedious, to detail particularly the different

* Medical Eſſays, vol. v. art. 12.

† Perhaps Dr. Alſton was led to conclude, that the gum exiſts in greater proportion than the reſin, from obſerving, that water poured on Opium will take up more than pure ſpirit of wine; but this ariſes from the gum's being capable of rendering a greater quantity of reſin ſoluble in water, than reſin can of gum in ſpirit of wine. See Preface to Lewis's Mater. Medica, p. 18.

experiments

experiments from which theſe concluſions are drawn. They are the reſult of the compariſon of ſeveral, made with the greateſt exactneſs poſſible. I cannot ſay they all perfectly agreed; nor will they be ever found to do ſo. But I have ſtruck a medium between them; and am certain, that for any medical purpoſe the concluſion is ſufficiently near the truth. The exact proportion of gum and reſin is, indeed, in this inſtance at leaſt, and probably in every other, of little importance to be known. It is of more conſequence, medicinally or pharmaceutically conſidered, to determine in which the virtues of the medicine reſide, and whether the principle which contains them be of a volatile nature or not. To aſcertain theſe points, recourſe was had to the following experiments.

Experiment XIII.

By ſubjecting a quantity of Opium to the action of diſtilled water, infuſing what it would not diſſolve in rectified ſpirit of wine, and precipitating what the ſpirit diſ-

ſolved

ſolved by the affuſion of water, I obtained a portion of reſin perfectly pure and free from gum.

Experiment XIV.

Having poured on a quantity of Opium ſome pure rectified ſpirit of wine, triturated them well together, and ſuffered it to diſſolve all it was capable of taking up, I ſubjected the reſiduum to the action of water; which was, after ſome days infuſion, filtered, and reduced to the conſiſtence of a hard extract by the heat of a water-bath. By this means I obtained the gum nearly free from reſin, but not ſo perfectly pure as the reſin in the former experiment was from the gum; for the reſiduum remaining after the action of the ſpirit of wine, and which was ſubjected to the action of water, neceſſarily contained in its moiſture a ſmall portion of what the ſpirit of wine was capable of diſſolving, which the water now took up by the intervention of the gum; and this ſmall relict of the preceding menſtruum could not be got rid of as in the former inſtance,

for

for the affuſion of ſpirit of wine will not ſeparate the gum in any quantity from water, though the affuſion of water will ſeparate reſin from ſpirit of wine, and leave it free of any principle which water can diſſolve.

Experiment XV.

The pure reſin of Experiment XIII. when rubbed between the fingers, had more of the peculiar ſmell of Opium than the gum; when chewed, it had no peculiar bitterneſs or pungency. The gum of Experiment XIV. had more the ſmell of extract of liquorice than Opium; to the taſte it was extremely bitter and pungent.

Experiment XVI.

Having diſſolved equal quantities of pure reſin and pure gum in equal portions of ſpirit of wine and water, I added to each a ſmall quantity of a ſolution of 20 grains of ſal martis in two ounces of water: the reſinous ſolution inſtantly ſtruck a dirty black colour, which was ſomewhat increaſed by

ſtanding; the gummy ſhewed but very little change.

From theſe experiments it appears, that the bitterneſs of Opium reſides wholly in the gum; and that the ſlight aſtringency it poſſeſſes is contained in the reſin. Many authors have been of opinion, that the narcotic and deleterious properties of Opium reſide wholly in the reſinous part; and that the gummy, if perfectly ſeparated therefrom, poſſeſſes all the anodyne and uſeful qualities of the medicine, and never occaſions the diſagreeable conſequences attributed to the reſin. Various conſiderations, however, ſo obvious as not here to require being particularly detailed, muſt lead us to conclude, that the diſagreeable effects aſcribed to the reſinous part of Opium are merely the reſults of the operation of the ſame power, which in other inſtances proves ſalutary and beneficial. Whether that power reſide wholly in the reſinous or gummy parts, or in both, is another queſtion, only to be determined by experiment. But that any ſeparation of the component principles

of

of Opium can prove of any utility in medical practice, or render its effects more beneficial and less dangerous, is, I am convinced, from a general view of the many experiments made on the subject, an erroneous opinion; and we may safely repeat with Sydenham—"Et quamlibet sint nonnulli, qui credulis persuadere velint, omnem fere narcoticorum, Opii præsertim ipsius virtutem, ab artificiosa ac debita quam soli adhibent præparatione pendere; qui tamen experientia judice certaverit, & tam simplicem succum a natura oblatum, quam ejus præparata cum diligenti observatione in usum frequenter revocaverit; nullum fere discrimen intercedere comperiet, & mirandos illos effectus, quos edit, nativæ ipsius plantæ bonitati atque excellentiæ, non vero artificis polydædali solertiæ, deberi certo sciet *."

As an acquaintance, however, with the seat and nature of the principle in which the virtues of Opium resides, may influence and direct us in its pharmaceutical treat-

* Opera, p. 106.

ment, I have endeavoured to approach precision in this point, as nearly as possible, in the manner detailed in the subsequent experiments.

Experiment XVII.

Of the pure resin procured by the means mentioned in Experiment XIII. I took two grains dissolved in a very small quantity of spirit of wine, at one in the afternoon, my pulse beating 70 in a minute:

In	5	10	15	20	25	30	35	40	45	50	55	60	75	90	minutes,
P. beat	78	78	80	78	78	76	70	72	66	65	63	65	63	66	

In 15 minutes, there was an evident increase in the fulness as well as frequency of my pulse; in 60, perceived a pleasing languor and drowsiness; in 90, this was so much increased, that I lay on the bed and slept for an hour; on getting up found myself languid, very thirsty, and affected with a slight vertigo and lightness of my head; on taking a glass of port wine all these symptoms were relieved, and I eat my dinner with as good an appetite as usual.

EXPE-

Experiment XVIII.

At twelve o'clock mid-day, my pulſe beating 70, took two grains of the pure gum of Experiment XIV. diſſolved in water; it produced no effect whatever, either on my pulſe or any other function.

Experiment XIX.

Imagining, from the preceding experiment, that the gum thus procured was nearly inert, I took, at half paſt two o'clock in the afternoon of the ſubſequent day, my pulſe beating 72 in a minute, *five* grains of it diſſolved in half an ounce of water.

In	5	10	15	20	25	30	35	40	45	50	55	60	65	75	90	105	minutes,
P. beat	72	72	74	76	80	74	72	72	72	68	66	64	66	66	62	66	

In 20 minutes, my pulſe was fuller as well as quicker; the heat of my body was raiſed, and I perceived a manifeſt increaſe of ſpirits; in 35 minutes, found a ſlight heavineſs and pleaſing languor coming on, the fulneſs of my pulſe diminiſhing; in 60 minutes, perceived my mouth rather dry; in

65 mi-

65 minutes, the dryneſs, conſequent thirſt, heavineſs, and other ſimilar ſymptoms gradually increaſing, with drowſineſs, and ſlight vertigo on attempting to walk; in one hour and a half, the heavineſs, vertigo, and other ſimilar ſymptoms were ſtill increaſing, pulſe between 60 and 70, and weaker—thirſt urgent. In this ſtate I ſat down to dinner; and it was obſerved by one who knew nothing of what I had taken, that I looked as if very drunk. I could not touch above a few morſels of ſolid food, but took a glaſs of wine and two of punch, which I imagined ſomewhat relieved me; but, in about an hour more, the drowſineſs and vertigo became ſo conſiderable I could ſcarcely walk, and my ſtomach turning ſuddenly ſick, I vomited up the whole of its contents, which immediately relieved every diſagreeable ſymptom; the drowſineſs, however, afterwards increaſed, and I ſpent the evening in a ſtupid doſing way, though I took a few diſhes of tea—pulſe in general from 62 to 70—ſtomach ſickiſh—ſleep in the former part of the night diſturbed and con-

fused; towards morning, more sound and refreshing—body costive for 36 hours afterwards.

Experiment XX.

Having, by means of gum arabic and sugar, united some pure resin of Opium with distilled water in the proportion of three grains to one ounce, I injected a portion of it between the skin and muscles of the posterior extremities of a frog; the limb was rendered paralytic in about 15 minutes; the animal was killed by it in an hour and a half.

Experiment XXI.

Employing the gum in the same manner, it produced the same effects, but more slowly than the resin.

From comparing these experiments it sufficiently appears, that the gum of Opium separated from the resin as perfectly as it can be by the usual fluids, though inferior to the latter in point of force, yet retains a sufficient degree of power to affect the syf-

tem considerably, if given in increased doses. Whether this be owing to the principle which gives activity to Opium being possessed, though in different proportion, both by the gummy and resinous parts, or to the impossibility of perfectly depriving the gum of every portion of resin by the effusion of different menstrua, may seem doubtful; though to me it appears more than probable, that the resinous part is that alone which possesses activity, and that the gum serves principally to give it solubility in the gastric and intestinal fluids. To this conclusion we will, I think, be led by the following considerations:

1st. When the resin and gum are separated in the usual imperfect manner, the activity of the former is considerably greater than that of the latter.

2dly. The gum thus separated must retain no small portion of resinous matter, both for the reason assigned in Experiment XIV. and because the gum, by its natural attraction for, and union with the resin, will detain a part of it, preventing the al-

cohol from taking up the whole it would otherwife diffolve. And,

3dly. We have a ftill ftronger argument, from confidering the confequences of the continued boiling a watery folution of Opium, as in the preparation of the extract recommended by Mr. Baume *. This operation, which I have had neither time nor patience to repeat, is as follows :—" Let five pounds of good Opium, cut in pieces, be boiled about half an hour in twelve or fifteen quarts of water; ftrain the decoction, and boil the remainder once or twice in frefh water, that fo much of the Opium as is diffoluble in water may be got out; evaporate the ftrained decoction to about fix quarts, which being put into a tin cucurbit, placed in a fand-bath, keep up fuch a fire as may make the liquor nearly boil for three months together, if the fire is continued day and night; and for fix months, if it is intermitted in the night, filling up the veffel with water in proportion to the evapora-

* Elemens de Pharmacie, Medical Mufeum, vol. ii. No. 61.

tion, and ſcraping the bottom with a wooden ſpatula, from time to time, to get off the ſediment which begins to precipitate after ſome days digeſtion. The ſediment needs not be taken out till the boiling is finiſhed, at which time the liquor is to be ſtrained, when cold, and evaporated to an extract of a due conſiſtence." In the author's remarks on this preparation, he obſerves, that "Opium is compoſed of a gummy ſubſtance, a reſinous matter, an eſſential ſalt, and a thick eſſential oil; ſuch are at leaſt the products that manifeſt themſelves during the digeſtion, and ſuch are the ſubſtances that reſult from its decompoſition. The eſſential oil of Opium has a conſiſtency like that of butter ſemifixate; it is not at all volatile in this ſtage, at leaſt I have remarked it did not riſe by diſtillation, as the eſſential oils that are extracted from other vegetables; yet it becomes attenuated and ſubtilized by digeſtion, and a great part is thereby rendered volatile. One would be induced to believe, that the flexibility of its reſinous matter is derived from the oil it

 contains,

contains, and that it ſerves as an intermedium to unite together all the different principles that conſtitute the ſubſtance of Opium.

"After three or four days digeſtion, this oil, which is produced by the decompoſition of the reſin, begins to float on the ſurface of the liquor, and to form thereon, by the cold, a fatty reſinous covering, which ſticks to the fingers ſomewhat like turpentine; it is about the thickneſs of a ſhilling, and of a grey or aſh colour, and does not diſappear until the end of the firſt month of the digeſtion; but it always leaves behind ſome clouds until the third month; this proceeds from thoſe particles of the reſin that ſeparate themſelves the laſt from the gummy ſubſtance; thoſe oily clouds diſappear entirely, except when the liquor is hot, whereas they appeared before as well cold as when hot; the reſinous ſubſtance of the Opium loſes, likewiſe, its fluidity, quits the gummy ſubſtance, dries apace, and decompoſes itſelf entirely. This reſin ſticks, in the beginning, to the bottom of the

the veſſel, when cold during the night; but as ſoon as the veſſel is heated, it leaves immediately the bottom; it preſerves, during a long time, the form of a reſin, grows ſoft by heat, is apt to crumble, and falls to powder when cold, and remains in powder, nor reunites into a maſs, having undergone a total decompoſition.

"One may ſuppoſe with ſome appearance of reaſon, that the ſmell and narcotic quality of the Opium reſide chiefly in its oily and reſinous principles, ſince in proportion as thoſe principles ſeparate one from the other, the Opium loſes its virulent and nauſeous ſmell and its narcotic property, retaining only its more lenient anodyne power, and occaſions no more thoſe deliriums that are very often produced by the crude Opium. In fine, when the digeſtion is finiſhed, the liquor has no taſte that approaches to that of Opium; in this ſtate its taſte ſomewhat reſembles that of any inodorous and inſipid vegetables when half boiled *."

* This tranſlation is taken from the Medical Muſeum, vol. ii. p. 398.

By the continued heat and boiling employed in this operation, it appears, then, that the resinous matter is decomposed, and perhaps more completely separated from the gum than by any other process whatever; and we accordingly find, that the gummy extract thus prepared, possesses scarcely any of the powers of Opium. The solitary case adduced in support of its efficacy, as well as its sensible qualities, would lead us rather to suspect that it is totally inert. In the case alluded to, that of a lady afflicted with convulsive motions of the stomach, and vomitings, it was given in doses of *fifty* grains daily, for *several years*, without producing any disagreeable consequences; but the circumstances appear only to shew, that its efficacy, as an opiate, was quite extinct; for the removal of such complaints after many years duration, cannot fairly be ascribed to an extract, whose exhibition was not attended with the slightest sensible effects.

It appears then probable, that the resinous matter of Opium is the active principle in

which its virtues principally reside: that part of it, however, which, in the observations on Baume's extract, is improperly termed the Essential Oil of Opium, appears to possess the greatest degree of strength and activity. This is particularly noticed by Neumann, whose sentiments respecting the component principles of Opium, coincide perfectly with those I have been led to form from many experiments and much consideration.

"The principles (he says) separable from Opium are, a resin, gum, a minute portion of saline matter, water, and earth. The resin is of two kinds, one more truly resinous, of a solid consistence, in its nature more fixed, and in its operation more sluggish; the other, softer and thinner, more volatile, and of a more speedy and powerful activity. The saline matter is of the acidulous kind, analogous to the essential salts of other vegetables; its proportion is so small, that it is not easily separable in its proper form, though it has sometimes happened, that actual crystals have concreted in a watery solution

lution of Opium. The resin, the gum, and the salt are very intimately combined together, insomuch, that all three dissolve almost equally well in water and spirit. It is probably to the saline principle in this and other vegetables, that this intimacy of union is in a great measure to be ascribed.

"The subtile soft kind of resinous matter, discovers itself, in a great measure, in the bare watery solution of Opium, generally arising to the surface in the form of a fat unctuous frothy substance; this is the strongest and most active part of the Opium; a few grains are sufficient to kill a dog, who could bear a whole dram of crude Opium. From a pound of Opium, we may collect two or three drams of this balsam-like substance; but we are not to imagine that this is the whole quantity which the Opium contains; beside what thus spontaneously separates, a part remains combined with the rest of the juice, and probably is the principle, or the direct seat of the principle which gives activity to the whole *."

* Chemical Works translated by Lewis, p. 308.

With

With refpect to what Neumann here advances, as to the fuperior activity of this part of Opium, I have been content to take his affertions for granted. His veracity has never been called in queftion; and as I am convinced no advantage can poffibly arife in medical practice from any fuch feparation of its inert and concentration of its active principles, I have been averfe to any experiments with it on the human frame, which may be attended with danger, and equally fo to any trial of its effects on brutes, which might juftly be deemed inftances of *unneceffary* cruelty.

As an acquaintance with the degree of volatility which its active part poffeffes, is a point of fome confequence, and fhould ever be kept in view in the pharmaceutical treatment of Opium, more pains were taken to acquire fome knowledge on the fubject, by repeating oftener than once the fubfequent experiments.

Experiment XXII.

Having triturated one ounce of Opium with twelve ounces of highly rectified ſpirit of wine, I ſuffered it to ſtand for ten days; and putting it into a ſmall alembic, expoſed it to a ſmart heat, and drew off eight ounces of ſpirit perfectly pure, free of the peculiar taſte and ſmell of Opium, not turning milky when mixed with water, and ſhewing no ſign, in ſhort, of any impregnation whatever.

Experiment XXIII.

Having triturated one ounce of Opium with one pound of diſtilled water, I put them into an alembic, and applying a briſk fire, drew off about four ounces of a clear fluid, which was extremely nauſeous, poſſeſſing a good deal of the peculiar ſmell, though but little of the taſte of Opium, but being much more diſagreeable. This was particularly the caſe on its being firſt drawn off; it became leſs ſo on ſtanding for ſome days. I then added, through a perforation in the

head

head of the alembic, four ounces of diſtilled water, and again drew off an equal quantity ſimilar in appearance to the firſt. This was repeated until two pounds of water had been added and drawn off; the laſt portion of which, being forced over by a very ſmart heat, ſhewed on its ſurface a thin film, but ſo inconſiderable in quantity, it could not be collected; it adhered to the ſide of the glaſs in which the liquor was received; and when rubbed with the fingers, felt and appeared like a butyraceous or tallowy matter: the glaſs could not be well cleaned by water, though eaſily and completely by ſpirit of wine. There now remained about eight ounces of fluid in the alembic, when raiſing the fire ſtill further, ſo as to make it boil up to the head, I drew off about two ounces of a whey-coloured fluid, more nauſeous than any before obtained, and leaving ſtill more of the butyraceous matter adhereing to the glaſs. The liquor remaining in the alembic, on cooling, though intenſely bitter, had loſt a great deal of the peculiar ſmell and taſte of Opium.

um. Having filtered it, I infuſed the reſiduum in four ounces of ſpirit of wine, and found it extracted about five ſcruples of reſin, which I obtained by evaporation in a water-bath.

Experiment XXIV.

Of the reſin thus obtained, I took two grains diſſolved in a ſmall quantity of alcohol; it produced no effect on my pulſe; I thought I perceived my head a little affected; but this was at any rate very inconſiderable.

Experiment XXV.

Three grains of the ſame reſin raiſed my pulſe from 70 to 75, and affected my head ſlightly. They were taken at mid-day, and did not induce ſleep, or impair my appetite for dinner.

From the four preceding experiments, and from comparing the 24th and 25th with the 17th, it is ſufficiently obvious, that the active principles of Opium, though fixed in the heat of boiling ſpirit of wine, are volatile in that of boiling water; and that in

in all the preparations of this medicine, its efficacy will be impaired in proportion to the degree of heat it is expoſed to.

As Opium loſes a conſiderable portion of its activity in the heat of boiling water, and as the water drawn off is extremely nauſeous, and poſſeſſes a good deal of its peculiar ſmell, it would be but natural to conclude, that the fluid thus diſtilled muſt be of a very active nature, and poſſeſſed of the remarkable properties which the medicine it is drawn from exerts. We have, beſides, an experiment related by Dr. Leigh, in which fifteen drops of this water are ſaid to have excited ſuch vomiting, as deterred him from making any further experiment upon it. Deſirous of aſcertaining what the real powers of the diſtilled water of Opium were, I had recourſe to the following experiments:

Experiment XXVI.

About twelve o'clock, being in perfect health, took *ten* drops of the diſtilled water of Opium, which produced not the ſmalleſt

effect. The ſucceeding day I increaſed the doſe to *twenty*, and on ſubſequent days it was augmented to *half an ounce*; but in no inſtance did it ſhew the ſlighteſt appearance of activity.

Experiment XXVII.

I gave to a healthy young man *two ounces* of the diſtilled water, which produced no effect whatever.

Experiment XXVIII.

Having mixed *ſix ounces* of the diſtilled water with ſome bread and milk, gave it to a dog; he ſhewed no ſigns of uneaſineſs, drowſineſs, or any other of the uſual effects of Opium.

Experiment XXIX.

Having ſeparated, by inflation, the ſkin and muſcles of the poſterior extremities of a frog, I injected ſome of the diſtilled water between them; it neither produced inſenſibility of the limbs, nor any other effect.

From

From theſe experiments it appears probable, however we may account for the fact, that the diſtilled water of Opium is perfectly inert. That this does not ariſe from the active principle of the medicine being of a fixed nature, is evident from its ſtrength being conſiderably impaired by being ſubjected to diſtillation; it is therefore probably owing to the activity of the volatile reſin being deſtroyed by the degree of heat neceſſary to its elevation.

With regard to the diſtilled water of Opium, I ſhall make but one obſervation more; it is reſpecting the following experiment related by Dr. Leigh:

"To a ſmall quantity of water drawn off from Opium by diſtillation, was added a ſufficiency of ſoluble tartar to ſaturate it. After ſtanding twenty-four hours, I examined it with a magnifying glaſs, and diſcovered evidently ſmall globules of oil floating on the top of the water; to the taſte theſe were extremely pungent, attended with a peculiar ſenſation, which is common to Opium alone. I was induced to adopt this

method for the following reafons:—Suppofing that the water which I had drawn off, from its fmell and tafte, poffeffed a quantity of oil, the particles of which might be fo intimately combined with it as to render them invifible, I thought it neceffary to add fuch a body as might unite with the water, and fet the oil at liberty. It is well known, that water and foluble tartar have a great affinity; and thus we find, that as foon as the water was faturated, the oil rofe to the top *."

Refpecting this experiment I fhall only obferve, that I repeated it very exactly, but never could perceive any fuch appearances as thofe here defcribed. In the diftilled water of Opium, the only trace of oily matter I could ever difcover, is that related in Experiment XXIII.; nor can I eafily conceive how the tafte of fmall globules, only difcernible by the microfcope, could be fo exactly afcertained.

The faline matter of Opium is fo fmall in quantity, fo difficult to be feparated pure,

* Experimental Inquiry, p. 56.

and ſo inert, that I did not deem it requiſite to examine more particularly than others have done into its nature and qualities. The old opinion, that the activity of the medicine depends upon the volatile ſalt it was ſuppoſed to contain, is ſo univerſally deſerted as to require no refutation. It is unneceſſary to remark, that in its natural ſtate it contains not an atom of ſuch an ingredient.

From the whole of the facts, authorities, and experiments adduced, we may, I think, fairly lay down the following poſitions:

1. Opium is compoſed of a gum, a reſin, an eſſential ſalt, and of earthy indiſſoluble impurities.

2. The quantity of gum and reſin is nearly equal; the proportion of the ſalt very inconſiderable; the earthy impurities amount to *three* parts out of *twelve*.

3. The gum, when *perfectly* ſeparated from the reſin, is diveſted of the peculiar properties of Opium, poſſeſſes no degree of aſtringency, but retains the whole of the bitterneſs of the medicine.

 4. The

4. The refin is of two kinds, one more fluid, fixed in the heat of boiling fpirit of wine, but capable of being volatilized in that of boiling water, efpecially if it be continued for a confiderable length of time; the other portion is more fixed, and not capable of being elevated by any continuation of boiling water heat. The refinous matter is void of bitternefs, but poffeffes as well the whole of the aftringency of the medicine, as of the peculiar and narcotic properties for which it is celebrated. The activity of the refinous matter feems to be deftroyed by the heat neceffary to its elevation, as the diftilled water of Opium is perfectly inert.

5. The fmall portion of effential falt which Opium contains, is analogous to that of other vegetable fubftances, and poffeffed of no peculiar properties.

6. Whether it be occafioned by the prefence of the faline matter, or by the attraction between the gum and refin, the union of both is fo ftrong, that the refin cannot be perfectly feparated from the gum by the action of different menftrua.

7. Any ſuch ſeparation of the component parts of the medicine, is of no uſe whatever in medical practice.

CHAP. IV.

A concise View of the various Opinions entertained by different Writers respecting the Mode of Operation of Opium.

IN this chapter I shall briefly enumerate the principal opinions which have been entertained, with respect to the manner in which Opium produces the effects mentioned in the preceding pages, and those observed in different morbid states of the system: but, confining myself merely to the part of an historian, shall defer entering into the merits of any; reserving for that purpose succeeding chapters. Nor shall I trespass on the reader's patience by particularly enumerating the different *modifications* of opinions entertained by every celebrated name in medicine; for, however they may differ

differ in ſome particulars, they will all be found referable to a few general heads.

Galen, who ſeems to have been the firſt to exhibit Opium, though with a ſparing and timorous hand, attempted, on thoſe principles by which he was apparently led in his reaſonings in other inſtances, to explain its wonderful, and ſeemingly deleterious properties. Medicines, it is known, he divided into *cold* and *hot*; and Opium, he aſſerted, was cold in the fourth or greateſt degree. The proofs he aſſigned in ſupport of this aſſertion, ſeem, as far as they can be collected, to have been deduced from its power of inducing ſleep, ſtupor, and other ſimilar affections; all of which, he aſſerted, were ever the conſequences of the exhibition of cold bodies; and from the nature of thoſe ſubſtances which he deemed neceſſary to be joined with Opium in order to correct the ſuppoſed intenſity of its frigidity; all of which ſubſtances he ranked in the claſs of hot medicines.

Long was the authority of Galen predominant in the ſchools, both among his co-

temporaries,

temporaries, immediate ſucceſſors, and the firſt profeſſors of the healing art after the reſtoration of learning. With his other tenets this mode of explaining the properties of Opium was implicitly acceded to; and one principal conſequence reſulting therefrom was, that it was always neceſſary to exhibit with Opium ſome medicines of a hot nature, to correct the intenſity of that property by which it was ſuppoſed to act *. To this circumſtance do we owe thoſe famous medicines, the Philonium, Theriaca, and Mithridate; ſo long from a veneration for antiquity retained, and at length ſo juſtly exploded.

As the chemiſts were the firſt that openly oppoſed the authority of Galen and his commentators, ſo their opinions on many medical queſtions were nearly oppoſite.

* See Galen. lib. viii. de Simpl. Med. Facult.——Trallian. lib. iii. cap. 1.——Actius. Tetr. 3. ſerm. i. cap. 32.——Oribaſius, lib. ii. de Virtute Simplic.——Tallopius de Comp. Med. cap. 8.——Ternel. lib. v. and vi.——Avicenna, lib. ii.——The works of the Arabians, &c.

With reſpect to the nature and mode of action of Opium they were diametrically ſo; for though Galen had placed it among the coldeſt bodies known, the new ſect aſſerted it was of a hot nature, and that all its effects were to be attributed to its heating properties alone*. Theſe poſitions they ſupported by arguments drawn from its oſtenſible qualities, as its ſmell, bitter taſte, inflammability, &c. from many of its effects on the human body, as its exciting heat, thirſt, and itching, intoxication, and other effects always conſequent to the exhibition of medicines of a hot nature.

Such were the oppoſite opinions avowed and ſupported by theſe two contending ſects, while they continued to divide the medical world. Many, however, were not wanting who ſteered a middle courſe, aſſerting that Opium was compoſed of particles in their nature different, ſome poſſeſſing a hot, and

* See Wedelin's Opiologia, cap. 6.——Felix Platerus, Caſpar Banhin, Helmont. Duumvirat.——Sala Opiologia, cap. 3.——Matthiolus, lib. iv. cap. 60.

others

others a cold quality; that the cold were more in number than the hot, and that, in consequence of this composition, the effects which were to be attributed to the hot particles were slight and transient; and those produced by the cold, violent and permanent *.

When, after many new discoveries in philosophy and physiology, and a more intimate acquaintance with the animal œconomy, new systems were introduced, and the principles of mechanical philosophy were applied to medicine; the speculations of physicians with respect to the action of Opium were newly modified, and founded on data similar to those which supported their reasonings in other instances. For although, in the writings of more ancient authors, we sometimes meet with what may be termed a mechanical explanation of the properties of Opium; it was not till the

* See Capivacius Tractat. de Venen. lib. vii. cap 9. ———Renodæus de Mater. Med. sect. x.———Casp. Hoffman. de Medicam. Offic.

period

period alluded to, that ſuch hypotheſes were minutely explained, or generally adopted.

Soon, however, as the mechanical mode of reaſoning became prevalent, the generality of phyſicians attempted to explain the action of Opium ſolely on its mechanical principles; and we find them, not only at the period during which it moſt flouriſhed, but for a conſiderable time after it had been in other inſtances univerſally deſerted, accounting for the effects reſulting from this medicine, by ſome of the following hypotheſes; which, though differing ſomewhat from each other, according to the fancy of the writer, were all founded in the main on ſimilar principles.

Opium was by ſome ſuppoſed to rarify, by others, to condenſe the blood. From theſe oppoſite aſſertions, however, conſequences perfectly ſimilar were deduced; in both inſtances the nerves at their origin it was imagined were compreſſed, according to the former hypotheſis, from the immediate rarefaction of the circulating fluid; according to the latter, from the free circulation of the

the blood through the minute veſſels of the brain being impeded by its condenſation, or coagulation. Of the nervous compreſſion thus ſuppoſed to be induced, all the more obvious effects of Opium, as inſenſibility, ſleep, ſtupor, &c. were reckoned the immediate conſequences; and the intenſity, or long continuance of this compreſſion was ſuppoſed to occaſion the loſs of life conſequent to a fatal doſe. Some, however, judiciouſly concluding, that the ſmall quantity of Opium uſually exhibited could have but little effect either in rarefying or condenſing the whole maſs of blood, and finding ſuch ſuppoſition not proved by any experiment, imagined it was merely conveyed by that fluid to the orign of the nerves, and, by its immediate action on them, produced the effects alluded to: while others, ſtruck with its very ſudden operation, ſo apparent in many inſtances, and finding the greater part of the doſe remaining in the ſtomach after theſe effects had been produced, aſſerted, that its more minute and volatile parts entered directly the extremities of

of the nerves dispersed on the stomach and intestines, and were through them conveyed to their origin in the brain.

With respect, however, to the immediate operation of this medicine on the nerves, after its arrival at their origin, we find a variety of opinions advanced; and, in forming them, unrestrained liberty generally allowed to the licentiousness of every warm imagination. In general, it was supposed that its ultimate particles, entering the minute cavities of the nervous fibres, disturbed, or totally impeded the free and equable motion of the nervous fluid, or animal spirits. But this leading idea was variously modified, the peculiar fancy of many writers explaining in a different manner its immediate operation in producing this effect.

By many it was attributed to the adhesive nature, and great tenacity of the ultimate particles of the medicine; by which the cavities of the nerves were for a time completely obstructed. By some, these particles were supposed to be in shape similar to hooks, arrows, &c. which sticking

in the internal membrane of the nervous cavity, drew its ſides together and thus obſtructed the paſſage: others we find attributing the effects alluded to, to its action on the nervous fluid itſelf; which was ſuppoſed to be condenſed or coagulated *. But it would be equally tedious and unneceſſary to detail, in a more particular manner, the diverſity of opinions of this claſs formerly promulgated; they have been too long and too juſtly neglected to require enumeration; and nothing further ſeems at preſent requiſite, than an account of the different opinions with reſpect to the operation of Opium entertained at the preſent day.

In detailing theſe I ſhall be equally, or even more conciſe than in my enumeration of the more antient opinions; for, as they neceſſarily require a particular diſcuſſion, the data on which they are founded, and the arguments adduced in their ſupport, will be

* For a particular account of theſe opinions, and the authors who ſupported them, ſee Tralleſius Uſus Opii, vol. i.

more properly confidered in fubfequent chapters.

By far the greater number of modern authors feem inclined to attribute the effects of Opium to its action on the nerves; on which it is generally faid to exert an immediate *fedative* power; without its being fpecified in what particular manner this fedative quality acts; Doctor Cullen, indeed, and others going fo far as to fay, that fedative powers produce their effects by deftroying the *mobility* of the nervous fluid. Yet, as among others, many effects are obfervable from the action of Opium, which are by all attributed to bodies termed *ftimulant*; many of thefe authors are inclined to attribute to it a ftimulant quality alfo, but of fo flight and tranfient a nature, as to be foon overcome by its oppofite, or the fedative power.

The ftimulant powers of Opium are, indeed, in numerous inftances, fo remarkable, that we find many writers comparing its mode of action and effects, to thofe of fubftances univerfally reckoned powerful

ſtimulants, as wine, alcohol, and the whole claſs of cordials; although its remarkable pre-eminence in allaying pain and irritation, and producing ſleep, inſenſibility, &c. has induced them, when attempting to explain its action, to aſcribe to it a direct and powerful ſedative quality. But the only writers or teachers, who, as far as I know, have expreſsly denied any direct ſedative power in Opium, and aſcribed all its effects to its ſtimulant quality alone, are the late Dr. Brown *, in his Elementa Medicinæ, Lectures,

* This author, poſſeſſed of a genius in many reſpects truly original, has advanced a ſyſtem of medicine, which appears to me, though I am very far from acceding to it in a great many particulars, to contain a number of ideas, new, bold, and well worthy particular conſideration. It is indeed to be regretted that the manner in which his doctrines were firſt given to the world, has tended, in a great meaſure, to impede their diffuſion and examination. The affected obſcurity of his ſtile, the very erroneous manner in which every edition of his Elementa was printed, the extravagant length to which he has extended many of his principles, and, above all, the looſe declamation and general abuſe which characterize his writings, and, as I have been informed,

tures, and other productions, and his pupils. Opium, according to this author, like every other medicine, acts primarily on the living principle, or, as he terms it, excitability of the ſyſtem; is poſſeſſed of a highly ſtimulant quality, and allays pain, induces ſleep, intoxication, and other ſimilar affections, by the exceſs of this power alone. Its remarkable pre-eminence, however, as an anodyne over other ſtimulants, he has not, as far as I can collect, attempted to explain.

Another opinion which remains to be mentioned is that lately advanced by the Abbé Fontana: from a number of experiments made with Opium on different animals, he poſitively aſſerts that it has no action whatever on the nervous ſyſtem, or vital ſolids; and that all its effects are to be attributed to changes induced by it on the blood. The particular nature, however, of theſe changes, he appears neither to attempt

informed, his lectures, have contributed, in a conſiderable degree, toprevent their being generally well underſtood or impartially conſidered.

to ſpecify, nor to anſwer the many objections urged by preceding writers againſt explanations of a ſimilar kind.

Such appear to have been the principal theories which have prevailed on the ſubject we are engaged in; and, though each in turn has had its numerous advocates, we find many phyſicians ſo far diſſatisfied with the explanations reſulting from them, as ever to be ready in acknowledging their ignorance with reſpect to its real operation, and willing to attribute the effects it produced to ſome peculiar and undiſcovered properties; and, notwithſtanding an explanation of its action has been attempted by a conſiderable proportion of the many medical writers who have flouriſhed ſince the very firſt ages of the ſcience, as appears from the preceding part of this chapter; yet it will be ſufficiently evident, from a little conſideration, that the ſeeming variety of opinions on the ſubject, however apparently different, may be reduced, with propriety, to three claſſes only. The

First, Containing the opinions of thofe who afcribe its effects to changes induced by it in the blood. The

Second, Of thofe who deduce them from its action on the living principle as a fedative, or fedative and ftimulant conjoined. And the

Third, Comprehending the fentiments of fuch as attribute to it the properties of a ftimulant alone.

For in what material circumftance do the difputes, with refpect to its *hot* and *ftimulant*, its *cold* and *fedative* qualities, materially differ? Are not the fentiments of many moderns, who deem it poffeffed of both a ftimulant and fedative power, fimilar to thofe of more ancient phyficians who affigned to it hot and cold qualities in different proportions? And will not a little attention fufficiently evince, that the mechanical conjectures of many, as to the particular manner in which it was fuppofed to obftruct the nervous canals and motions of the animal fpirits, do not vary effentially from the fentiments of thofe moderns, by

whom its ſedative effects are aſcribed to a diminiſhed mobility of the nervous fluid?

As the ſentiments, therefore, of every writer on the ſubject, ſeem referable to one or other of theſe three claſſes, we ſhall conſider them in the order laid down, aſſigning to the *firſt* the following chapter; and, in the next, conſidering the *two* latter; for the proofs, experiments, and arguments, which ſeemingly favour one opinion, will throw the greater light upon the ſubject, and be beſt contraſted, if ſtated and examined together.

CHAP. V.

Of the Opinions of those who ascribe the Operations of Opium to changes induced by it in the Fluids.

THE antiquated opinion, that the effects of Opium, whether medicinal or deleterious, are occasioned by its procucing any change in the circulating fluids, has of late years been so universally deserted, that it would appear almost unnecessary to attempt its formal refutation, had not a modern writer of eminence, the Abbé Fontana, embraced the opinion, and attempted to support it by a number of experiments. The nervous system, he asserts, is totally insensible to the action of Opium; injected into the blood vessels, it produces considerable

able and viſible changes; and to ſuch alone are all its effects to be aſcribed *.

In inveſtigating the propriety of the ſentiments of thoſe who aſcribe the effects of Opium to its operation on the fluids, I feel myſelf in ſome meaſure neceſſitated to notice an opinion of late maintained by the ingenious Mr. Hunter reſpecting the *vitality* of the blood, or its being poſſeſſed of a living principle; as to ſome perhaps it may ſeem probable, that, although Opium, in the quantities uſually given, is incapable of effecting any ſenſible change in the maſs of blood, it may, if ſuch a principle exiſt, pro-

* The experiments and reaſonings, by which he endeavours to eſtabliſh theſe ſentiments, are detailed in a work entitled, *Traité ſur le Venin de la Vipere, ſur les Poiſons Americains*, &c. 4to. Florence, which I firſt ſaw in the year 1788. I have it not before me at preſent, having been diſappointed by the perſon employed to procure it from London; and am therefore neceſſitated, in examining his reaſonings, to truſt to my own memory, to ſome notes I took down on peruſing it, and to the abſtract of its contents given in the Medical Commentaries for the year 1787. By the aſſiſtance of theſe, I hope I comprehend his principles ſufficiently to warrant my attempting a refutation of them.

duce

duce its effects by operating thereon. To me, I muft confefs, this opinion of the blood's vitality, and the fentiments of thofe who afcribe the effects of Opium to its producing any fenfible or other changes in the mafs of circulating fluids, appear equally ill-founded; and, in order to prove this with fome degree of regularity, I fhall endeavour to fhew,

I. That the blood is totally void of any living principle.

II. That the arguments and experiments adduced to prove that the nervous fyftem is infenfible, and the blood alone fubject to the operation of Opium, are fallacious and inconclufive. And,

III. I fhall ftate fuch arguments and experiments as appear fufficient to prove, that the nervous fyftem, or vital folids alone, are liable to the operation of the medicine in queftion.

I. To give a definition of the word *life*, and to explain in what it effentially exifts, are

are attempts which, however free from difficulty they may appear at firſt view, have puzzled philoſophers of all ages. Perhaps the explanations long ago given by Cicero, approach as near the truth as any ſince deviſed:—"Inanimum eſt," ſays he, "omne "quod pulſu agitatur externo; quod autem "eſt animal, id motu cietur interiore et ſuo. "Nam hæc eſt propria natura animi atque "vis *." Indeed, if we take life in the moſt extended acceptation of the word in which it applies to the vegetable as well as the animal kingdom, leaving the operations of intellect out of the queſtion; we know no circumſtance which diſtinguiſhes animate from inanimate matter, but the capability which the former poſſeſſes of being excited into motion by the application of certain extraneous inanimate powers, or by the neceſſities of the animated organ. The motions thus excited vary according to circumſtances, being in ſome inſtances ſudden, convulſive, and viſible; as in the contractions of irritated muſcles, and the motions

* Diſput. Tuſcul. lib. i.

of

of the leaves of ſuch plants as the Mimoſa, Dionæa, Muſcipula, &c.; while in other caſes they are gradual, equable, and only to be diſcovered by continued attention; as in the growth of different parts of animals, and the approach of the leaves of confined vegetables, to the only inlet of light and air.

But however different the modifications of motion may be in the ſolid parts of animals and vegetables, we know not a ſingle inſtance of ſuch motions being excited in any fluid of either tribe, except by the intervention and contraction of the ſolid parts. Separate a muſcle from an animal, and it will contract on being irritated; draw a cup of blood, and no irritation can rouſe it into action; prick the coat of a blood veſſel, ſenſation of pain and motion follow; paſs a ſharp inſtrument into the blood it contains, neither ſenſation or motion ſucceed. Did the exility of their veſſels allow us to make the ſame experiments on the veſſels and fluids of many vegetables, the ſame conſequences would no doubt enſue; ſo that judging of the preſence or abſence

of

of life, or a living principle, by the capability or incapability of motion, we may ſafely aſſert, that the ſap of the vegetable, and the blood of the animal kingdom, are both equally deſtitute of a living principle; and that the latter only ſerves to diſtend, excite into action, and nouriſh the vital ſolids, to afford and convey the neceſſary matter for various uſeful ſecretions, and to generate and equably diffuſe the neceſſary degree of animal heat.

Whether it has been from conſidering the queſtion in this point of view or not, one fact is certain, that from the time of Harvey, who ſupported the idea of the blood's vitality, to that of Mr. Hunter who revived it, philoſophers and phyſicians have very generally acquieſced in the oppoſite ſentiment. Where ſo great a majority have for a long time coincided, the *onus probandi* undoubtedly lies on Mr. Hunter and his adherents; and if, beſide the facts and arguments above ſtated, and the authority of the many againſt the opinion, it be ſhewn that the arguments in its favour are inconcluſive

clusive and fallacious, we shall not hesitate to decide that the blood of animals is totally inanimate.

The arguments which have been employed, are as follows *:

1. Blood unites living parts, as the juices of vegetables produce an union; were either dead, they would act as stimulants and prevent it.

2. It becomes vascular, as in the coagula formed in the extremities of divided arteries.

These two arguments may be considered together; the answer to each is similar. It is certain, that when the solid parts of a living animal are divided, and the lips of the wound are closely laid together, they will unite by the inosculation of the divided vessels, and perhaps by the formation of new ones. But although blood be in this instance effused, and vessels shoot through it; and although the coagulum formed in the extremities

* These arguments of Mr. Hunter are taken from vol. ii. of the Edinburgh Medical Commentaries, and the Encyclopœdia Britannica, art. *Blood*.

extremities of divided arteries fometimes becomes vafcular, I cannot fee how either fact proves the exiftence of a living principle in that fluid; it appears to me to be only the medium through which the divided, inflamed, and irritated living veffels extend their extremities. Can an inftance be fhewn, where effufed or coagulated blood, fo fituated as not to be in reach of thefe veffels, ever became vafcular? In aneuryfmal facs no fuch appearance has ever been difcovered: they are generally lined by a membrane which prevents fuch communication. Mr. Hunter fays, veffels are not formed in thefe inftances, becaufe they could be of no ufe to the animal. Of what ufe are the new veffels which connect the inflamed pleura to the furface of the lungs, or the convolutions of inflamed inteftines to each other? Are they not to be confidered as morbid appearances, and detrimental to the animal? When the parts juft mentioned are inflamed, an effufion of part of the blood takes place on their furface, through which the veffels, which poured

poured it out, ſhoot their extremities, inoſculate, and finally connect the contiguous membranes. Are we from this to conclude, that a fluid thus ſecreted, in conſequence of a *morbid* affection, is *alive?* Mr. Hunter introduced the teſticle of a cock, ſmeared with blood, into the belly of a hen, and found it, on injecting the animal, connected, by new-formed veſſels, with the ſurface of the liver. Has Mr. Hunter tried, whether introducing a ſimilar ſubſtance, ſmeared with warm milk, would not as often adhere as when covered with blood? The chance of its ſucceeding, for I look on the matter as accidental, would, I imagine, be as great in one inſtance as another. The membranes of the gravid uterus, termed *decidua*, and *decidua reflexa*, are ſaid, by many eminent phyſiologiſts, to be formed by an exudation from the internal membrane of the uterus, through which new veſſels ſhoot. Are we thence to aſſert, that the fluids which moiſten the internal ſurface of this cavity are animated? Mr. Hunter aſſerts, that all fluids through which veſſels ſhoot, *muſt* be

animated;

animated; this is, however, mere aſſertion; no proof is given in its ſupport; and the very reverſe appears from more than one fact. The umbilical veſſels of a chick ſhoot their extremities into the albumen, and abſorb it for the nouriſhment of the embryo; and in proportion as the liquor is exhauſted, they extend themſelves ſtill further to reach the remaining portion. Are we from this to conclude, that the inert liquor which nouriſhes, is equally alive with the veſſels which convey, and the animal which requires ſuch nouriſhment? In ſeveral inſtances, eſpecially in the performance of the flap operation, as recommended by my ingenious friend, ſurgeon O'Halloran *, two ſuppurating ſurfaces laid together, will unite and firmly adhere by veſſels which certainly muſt ſhoot through the pus they are covered with. But are we from this to conclude, that all purulent matter is alive? All theſe queſtions, upon Mr. Hunter's

* See Treatiſe on Gangrene and Sphacelus, with a new method of amputation, &c.

principles,

principles, muſt be anſwered in the affirmative; but the inadmiſſibility of ſuch affirmations ſtrikes us at one view. In many controverſies, eſpecially where abſolutely negative proofs are ſcarcely to be expected, the *argumentum ad abſurdum* is the moſt convincing that can be employed, and ſeems peculiarly applicable to the preſent diſcuſſion.

3. Blood drawn from a vein is of equal heat in the hotteſt and coldeſt atmoſphere.

Allowing this to be a fact, I cannot ſee how it gives a ſhadow of ſupport to the opinion in queſtion. A conſtant equability of temperature is neither the reſult of, nor neceſſary to, the exiſtence of a vital principle, as is proved by the temperature of different animals, varying, though not in exact proportion, with that of the medium in which they live *. When the human frame is expoſed to a high degree of heat, the conſequent increaſe of evaporation from its ſurface muſt, from the abſorption of heat,

* See Martine's Eſſay on the various Degrees of Heat in Bodies, ſect. 40, 41, 42.

neceſſarily tend to keep its temperature lower than would otherwiſe be the caſe. When the body is placed in a cooler atmoſphere, the conſequent decreaſe of evaporation will produce a contrary effect: whether it be entirely owing to theſe circumſtances alone that the heat of the human body is pretty equable in all variations of the atmoſphere, or to them conjunctly with ſome modification of the action of the ſanguiferous ſyſtem, we are too ignorant of the cauſes productive of animal heat to be able to determine. We are at any rate ſtill ſo ignorant of the real mode of its production, that any argument reſpecting the vitality of the blood, drawn from its temperature, muſt neceſſarily be fallacious.

4. Blood is capable of being rouſed into action by ſtimuli, as it coagulates from expoſure to the air. The more it is alive, that is, the more healthy the animal is, the ſooner it coagulates; and, e contra, the more it has loſt of the living principle, as in inflammations, the leſs ſenſible it is, and it coagulates the later.

It has been above aſſerted, that blood cannot be rouſed into action by any ſtimulus; nor does the coagulation of the blood, from expoſure to air, by any means contradict the aſſertion. The ſeparation and conſequent motion of its parts is merely a chemical effect, occaſioned, probably, by air acting on it as a coagulant *. After this motion has once been produced, the particles remain in the ſame ſituation till putrefaction commences. How different from the vital motions of the ſolids of plants and animals! which, when ſtimulated into action, recover their former poſition, and can be repeatedly rouſed into ſimilar motions by ſimilar applications, till the vital principle be entirely loſt. But let us ſee how far the admiſſion of this principle as a proof of vitality would carry us. Milk, when taken from its veſſels, ſeparates, as well as blood, into its different conſtituent parts; the cream riſes, the caſeous and ſerous particles ſeparate: if an acid be added to

* See Hewſon's Inquiry, p. 21.

fresh drawn milk, coagulation and a separation of its parts ensues instantly. Are these vital contractions? Are we thence to assert that milk is alive, and that acids act on it as stimulants? We must, if Mr. Hunter's principle be just; the cases are similar. Spirit of sal ammoniac, by the same argument, is alive, because a coagulum is instantly formed by its being mixed with alcohol; and lime water is alive, because its appearance is changed on exposure to the atmosphere *.

5. Blood preserves life in different parts of the body: if its nerve be cut, the limb does not mortify; if its artery be divided, it does.

These facts are just; the conclusion drawn from them does not follow. Blood is necessary to the preservation of life in a limb, because it is the chief existing power

* I should not be surprised to hear it asserted, that the florid colour produced in blood by the admission of air, is a proof of its vitality, and analogous to the redness excited by rubefacients, and other stimulants applied to the living system.

applied to the living principle; becaufe it is the only medium by which heat, fo neceffary to the life of warm-blooded animals, can be excited or diffufed; and, becaufe circulation is the only means by which decayed particles can be removed, and frefh fupplies conveyed in their place. The dividing of a principal nerve does not produce mortification, becaufe the divifion cannot deprive a limb of the entire of its living principle; for it is impoffible to divide *all* the nervous filaments which lead to any member, without entirely feparating it from the body; and the folids appear, from various confiderations, to poffefs a portion of living principle, independent of the nerves. If the artery leading to a mufcle be tied up, the power of motion is loft. Does any phyfiologift thence conclude, that the blood is the vehicle of that power which fets the mufcle in motion? No. We from this fact only determine, that both a free circulation, and nervous influence, are neceffary to mufcular action. Our conclufions refpecting

the preſent queſtion ſhould be ſimilar; both nervous influence and a free circulation are neceſſary to the perfect life of any member; and impeding the latter proves more ſpeedily fatal than diminiſhing the former, only becauſe it can be effected more univerſally and completely.

Another argument has been adduced in favour of the opinion, and is as follows:

6. The embryo originally conſiſted of a nutritious fluid, from which its different parts were afterwards formed. In this fluid the vital principle muſt have exiſted; and if it exiſted there, is it not probable that it exiſts in the fluid which ſucceeded it in office, the blood?

To this I anſwer: the firſt organized part of the embryo muſt have been a ſolid; for by the actions of a ſolid alone could this nutritious fluid be ſet in motion, and applied to the nouriſhment of the evolving parts. The heart is generally the firſt part obſerved, and always in motion. Whence it derived the ſpark of life, the principle of motion,

motion, we are entirely ignorant; we ever muſt remain ſo, nor ever can obtain elucidation in any argument, from chimerical enquiries reſpecting its origin.

Since writing the above, I have obtained from London an *Eſſay on the Vitality of the Blood*, by J. Corrie, M. D.; in which I do not ſee that the ſuppoſition of its exiſtence is by any means rendered more probable than before. Some of the arguments above employed in its refutation, have, I perceive by it, ſtruck Dr. Hendy; and have been publiſhed by him in an eſſay on glandular ſecretion; a work I have never peruſed. Any new arguments not above noticed, adduced by Dr. Corrie, may be comprized under the following heads:

7. "If blood, and jelly of ſtarch of the ſame thickneſs, and both of the ſame temperature as blood drawn from its veſſels, be expoſed, under ſimilar circumſtances, to the atmoſphere, the jelly will fall to the temperature of the air conſiderably ſooner than the blood. In this caſe the power of life in the blood reſiſts the action of the external

cold, and its ſlower cooling is a proof of its vitality *."

This concluſion does not follow. Bodies confeſſedly inanimate, by no means either heat or cool in proportion to their relative conſiſtence, or denſity. There is ſomewhat in the compoſition of different fluids that cannot *a priori* be diſcovered by any rule, which occaſions ſome to cool and heat faſter and ſlower than others †.

8. "Blood frozen, thawed again, brought to the ſame temperature as when drawn from the vein, and immerſed in a freezing mixture along with another quantity of the ſame blood, directly drawn from the ſame perſon, will freeze many minutes ſooner than that immediately from the vein. The blood having loſt its principle of life by

* P. 44. Though this and the ſucceeding arguments are marked with inverted commas, they are not exactly Dr. Corrie's words. They are, however, the exact ſentiments he advances, condenſed as nearly as poſſible in his own expreſſions.

† See Martine's Eſſay on the heating and cooling of Bodies throughout.

freezing, is incapable of refifting the application of cold, and confequently freezes long before that portion which has not been deprived of its life, and which therefore refifts it *."

To do away this, it appears fufficient to obferve, that as the facility with which confeffedly inanimate fluids cool and freeze, depends upon fomething unknown in their compofition; the change of compofition neceffarily induced by the cooling, freezing, thawing, and again heating, and confequently diffipating the more volatile parts of the blood in this experiment, affords a much more feafible explanation of the fact, than fuppofing it proceeds from a lofs of life. A cafe very much in point juft occurs to me.—Take equal quantities of plain water, boil one portion, and having reduced them both to the fame temperature, let them be expofed to a freezing cold. The boiled water will be converted into ice much fooner than the unboiled.—Who will

* P. 45, 46.

thence

thence conclude, that the water was alive, and its living principle killed by the excessive heat?

9. "If the blood acted on the heart merely as a chemical and distending stimulant, affecting its irritability, this stimulant quality ought always to be on the encrease to enable it to produce its effects; as it is an universal law, that a degree of insensibility to stimulants is always the consequence of their continued application: this, however, does not take place, and therefore the blood acts on the heart by its vital power, and not as dead matter *."

To this I answer: *First*, the data are false: for the heart does, by the continued application of the blood, become daily less sensible to its action, "Communis pulsuum frequentia erit in homine adulto, & sano, circiter 70 in horæ minuto; in fœtu forte, *duplo* & amplius velocior; in infante, primis vitæ mensibus vix infra 120; puero & adolescenti, paulatim lentior fit; in sene

* P. 60, &c.

decrepito

decrepito aliquando ad 58 & infra descendit †."

Secondly. If the heart and vessels are emptied of their blood, inanimate fluids, which can only act chemically and by distension, when poured into its cavities, excite them to similar actions; a strong presumption that they in all cases proceed from a similar cause.

Thirdly. The introduction of chemical stimulants into the blood, as when mercury is absorbed, quickens the heart's motions.

Fourthly. Confessedly inanimate substances, as the fœces and urine, which act only by distension and chemical irritation, produce through life repeated actions in the intestinal canal and bladder; though they also, as well as the heart, become gradually less sensible. This last objection alone, Dr. Corrie attempts to do away, but very unsuccessfully. In perusing this pamphlet, an argument has struck me which appears conclusive against the deductions drawn

† Gregorii Conspectus, sec. 445.——See also Halleri Elementa, et Prim. Lin. sec. 168, 954, &c.

from

from the coagulation of blood; a circumſtance on which more ſtreſs appears to be laid than any other. "Blood unconnected with the ſource of life, has a power of reſiſting, as other living bodies have, the action of cold, as long as this life remains *," or "till that quantity of life attached to it is *deſtroyed*; which takes place ſooner or later, according to the quantity of life, or the force with which the hurtful agents act †." Blood, therefore, once frozen is *dead*; its living principle has been *deſtroyed* by the hurtful power of cold. But it unfortunately happens, that if blood be frozen in its veſſels without the application of air, and afterwards thawed and expoſed to its action, coagulation will take place. "Blood can be even froze without coagulation taking place, and even after being thawed, by the action of air coagulate ‡." In this inſtance coagulation, ſuppoſed only to ariſe from the exertion of a living principle, takes place after this principle has, by their own con-

* P. 47. † P. 45. ‡ P. 31.

feſſion, been *deſtroyed.* *Pugnat ſententia ſecum* *.

II. We are next to conſider the facts and arguments adduced to prove, that the nervous ſyſtem is inſenſible to the operation of Opium, and the blood alone liable to its action.

It is to be obſerved that Fontana, at preſent the chief ſupporter of theſe opinions, concludes in the firſt part of his work, that Opium proves a poiſon by deſtroying the irritability of the muſcular fibre ; that is, by its action on the vital ſolids. In the latter part, however, he retracts this idea, and confines its operation to the blood only. In ſupporting this opinion he has neglected one very material point, viz. to refute the experiments and arguments of many phyſiologiſts of the firſt character, who have

* The truly original idea, of the blood obtaining its life from the lymphatic veſſels and glands, p. 91, 93. it ſcarcely ſeems neceſſary to diſcuſs. The ſpeculations reſpecting the operation of poiſons ariſing from their deſtroying the blood's coagulability, fall more properly under the ſucceeding heads.

maintained

maintained a contrary principle. Let us in this ſection conſider the validity of thoſe he advances, in proof of the juſtice of his own ſentiments. They may be reduced, I imagine, to the following heads:

1. Opium applied to the ſurface of the brain, to the crural nerves laid bare, or to the crural nerves detached from the vertebræ, produces no more effect than ſimple water; whether the nerve be cut or entire, whether it be covered with its proper coats, or whether its pulp be put in contact with the poiſon.

The deductions drawn from this fact appear fallacious for a very obvious reaſon. To enable any external powers to effect their uſual impreſſions on the nervous ſyſtem, it is not ſufficient merely to apply them to the ſurface of the brain, or to the nerves in their courſe; the nervous matter muſt be *organized* in a peculiar manner, to enable it to receive or be ſenſible to theſe impreſſions. Inſtances have not been wanting, where butter of antimony has been applied to the nerves in their courſe, with-

out

out producing any irritation or convulſive motion *. Would we thence conclude that the nerves are not the vehicles of ſenſation? Surely no perſon would expect that the portio mollis of the auditory nerve could be ſenſible to ſound, unleſs organized in a peculiar manner in the ear; or that the optic nerves could perceive light if expoſed to it in their courſe. That they cannot, although light be thrown upon the pulp and every other part of their ſubſtance, unleſs organized as in the retina, we have abſolute proof: they are both perfectly inſenſible at their entrance into the eye. Nay, although a nerve be organized in the neceſſary manner, a certain degree of tenſion, and fulneſs of the concomitant blood veſſels, is requiſite to enable it diſtinctly to receive the uſual impreſſions. From ſuch experiments, therefore, no ſatisfactory concluſion can be deduced.

2. The hearts of animals taken from the body, and conſequently deprived of blood,

* Haller. Memoires ſur les Parts irrit. &c. Exp. 207.

when immerſed in watery ſolutions of Opium, are not deprived of irritability, and the power of motion, ſooner than hearts immerſed in pure water.

This fact I am under the neceſſity of denying, although the learned Haller is of the opinion, as well as the Abbé Fontana. Whytt aſſerts, from the reſult of ſeven experiments which he particularly details, that the irritability and motive faculty of the heart is, as well as that of other muſcles, deſtroyed by Opium *. In determining this point it is to be obſerved, that if the hearts of a number of frogs, the animals on which ſuch experiments are uſually made, be ſeparated from the body, though no application whatever be made to any of them, ſome will die or loſe their irritability many minutes before others. It is therefore only from many experiments, and from taking a general view of the whole, that we can determine the point in queſtion; and after having tried the experiment upon a great

* Eſſays Phyſical and Literary, vol. ii. art. 20.

number

number of hearts, I do not hesitate to declare, that the irritability of that organ is destroyed by Opium long before it would naturally disappear. I think I have in general observed, that the larger the animal, the longer does its heart when separated from the body continue to vibrate; and in comparing the effects of plain water and Opium, we should, as nearly as possible, match the hearts employed in size. The liquors should also be of the same temperature, as a variation in this point will occasion a manifest difference. With these precautions in view were the experiments above-mentioned conducted. The following are the particular results of the last instituted on the subject.

Experiment XXX.

Having taken three frogs, one of a very large, another of the middle, and the third of rather a small size, I separated the hearts from their bodies, and immersed them in pure water. The first lived *forty-five* minutes; the second and third *forty* minutes. Im-

merſing the hearts of three frogs of nearly equal ſizes in a watery ſolution of Opium; the firſt lived but *twenty* minutes, the ſecond *eleven*, the third ſcarcely *ten*.

3. A ſolution of Opium injected into the veins, produces heavineſs, convulſions, and at length death. The blood is found congealed in the cava, in the heart and lungs, which are ſure cauſes of death without having recourſe to the nerves.

Of this argument the refutations are ſo obvious, that it appears ſurprizing it ſhould ever have been adduced in ſupport of the opinion. It is, in the firſt place, to be remarked, that the deductions are evidently founded on a falſe analogy. There is not the ſmalleſt ſimilarity between the application of a few grains of Opium to the internal ſurface of the ſtomach and inteſtines, and the injecting as many ounces of a ſolution of the ſame medicine, immediately into the jugular vein. In the firſt inſtance, the medicine is ſlowly diſſolved, mixed with the bile, pancreatic juice, and other fluids of the inteſtines; and if any particles do enter the

the blood veſſels, it muſt be gradually, and in very ſmall quantity. In the latter caſe, a quantity of fluid is introduced, which even if not charged with any medicine, would produce moſt dreadful effects: its ſudden admiſſion changes the craſis and compoſition of the circulating fluids. Hence many ſubſtances friendly to the animal frame, or at leaſt not poiſonous, when injected in a ſimilar manner, produce ſpeedy death. A decoction of Peruvian bark, or of Dragon's blood, will, in this way, kill as ſoon as a ſolution of Opium *. Who would thence conclude, that they muſt prove poiſonous by effecting ſome change in the blood when ſwallowed? and why, by reverſing the argument, conclude that Opium acts only on the blood? One concluſion is as fairly drawn as the other. The argument beſides, to give it any weight in the preſent controverſy, includes a ſuppoſition which is certainly falſe: viz. that medicines injected

* Friend. Emmenolog. cap. 14.

into the veins cannot affect the nervous ſyſtem, but muſt act entirely on the blood. The whole internal ſurface of the heart, arteries, and veins, is ſurely both ſenſible and irritable; ſo that the nervous ſyſtem is perhaps more conſiderably expoſed to the action of any medicine, when it is introduced into their cavities, than when applied in any other manner; and poiſons thus introduced, probably, prove in general ſo quickly fatal, both by being applied to the living principle more univerſally than in any other mode, and by ſuddenly changing, in conſequence of the mixture of the fluids which convey them, the craſis and compoſition of the blood.

Such are the principal arguments urged to prove that Opium operates on the blood alone. That they are at leaſt inconcluſive appears, I think, pretty evident. Let us next ſtate the facts and arguments on the other ſide the queſtion; none of which have been, and ſome of which appear incapable of being anſwered.

III. To

III. To give ſome regularity to this ſection, I ſhall, as in the former, arrange the arguments under diſtinct heads.

1. The very few minutes which Opium requires to affect the ſyſtem at large, after being taken into the ſtomach, affords nearly an abſolute proof, that theſe effects cannot be occaſioned by any alteration in the maſs of circulating fluids. It will be found, on conſulting the experiments ſtated in the ſecond chapter, that it often produces a manifeſt alteration in the pulſe in five minutes, a ſpace of time in which it is impoſſible it could paſs through the circuitous paſſage, by which alone it can reach the maſs of blood. In ſuch caſes, therefore, we muſt conclude, that its effects ariſe from its application to the internal ſurface of the ſtomach, and its conſequent operation on the living principle of the entire ſyſtem.

2. As we have ſhewn, that there exiſts not in the maſs of blood any portion of the vital principle, it follows, that the only manner in which Opium can affect the fluids, ſuppoſing it conveyed pure and unchanged

 by

by the lacteals, is by producing ſome chemical change in their component principles. Of this, however, there is neither proof nor probability; for when blood is drawn from a vein, and a ſmall quantity of an opiate ſolution is added to it, no change in its appearance can be obſerved. Indeed, when we conſider the relative proportion of the fluid to be operated on, and of the ſuppoſed agent, any ſuch change can ſcarcely be reaſonably expected. The body of an adult man contains about twenty-eight pounds of red blood *; twenty-eight drops of laudanum will produce conſiderable effects on his ſyſtem; but will any perſon aſſert, that one drop of laudanum can alter the conſiſtence or compoſition of one pound of blood? The entire argument, beſides, reſts on a ſuppoſition, of the truth of which no evidence has been given, viz. that medicines taken from the vegetable kingdom, paſs unchanged into the circulating maſs, notwithſtanding the mixture and dilution

* Haller, Prim. Lin. ſect. 149.

they undergo in the ſtomach, and the powers of digeſtion to which they are expoſed. The very reverſe of this ſuppoſition is in moſt inſtances, for many reaſons, more than probable *.

3. We have direct proof, that at leaſt the greater part of a doſe of Opium remains in the ſtomach and inteſtines, after the entire ſyſtem has been affected by it in the moſt violent manner. The learned Kann Boerhaave, having mixed three grains of Opium with ſome bread and milk, and given it to a dog, found, on diſſecting the animal after it had produced its effects, that the bread and milk were undigeſted, the pylorus ſhut, the ſtomach much diſtended, no chyle in the duodenum, and no lacteals perceptible in the meſentery. On opening another dog, to whom he had given thirty grains, which threw him into a profound ſleep, he found, on examining the ſtomach ſix hours after

* The blood of a man, who had for ſeveral days taken large quantities of bark, diſcovered no traces of its aſtringent matter on the addition of a chalybeate ſolution. Skeete on Bark, p. 134.

it had been taken, that it had ſcarcely loſt one grain of its weight *.

A pill of Opium given at night, is ſometimes vomited up in the morning, after having produced the deſired effect †. I have myſelf, when trying experiments on the different parts of Opium, often vomited up what appeared to me to be the entire quantity of the medicine I had taken, after its having affected me in a very violent manner.

4. In cold blooded animals, whoſe principle of life remains unimpaired for a conſiderable time after the circulation is ſtopped, if Opium be applied to the internal ſurface of the ſtomach and inteſtines, or between the ſkin and muſcles of the lower extremities, after the circulation has been completely ſtopped by cutting out the heart, the ſyſtem at large will be affected as ſpeedily and conſiderably as if the circulation had been allowed to continue in the natural

* Impet. faciens, ſect. 437.

† Van Swieten, Comment. in Boerhaav. aph. 229.

manner. This fact is clearly evinced by the experiments of Doctors Whytt and Munro, particularly detailed in the Physical and Literary Essays *. These experiments I have been at pains to repeat, and the results have been so similar, that a particular account of them seems here by no means requisite. As, however, it might be objected, that in these instances, although the circulation was stopped, absorption might have continued, and the blood be in this way affected; I advised the following experiment, which appears to me an *experimentum crucis* in support of the opinion, that Opium operates on the nervous system or living principle alone. It was several times repeated, and with similar event.

Experiment XXXI.

Having taken three frogs of equal size, which, for distinction sake, I shall number 1, 2, and 3, I treated them in the following manner:

* Vol. ii. art. 20. Vol. iii. art. 13.

No 1.

N° 1. After dividing the integuments quite round the body in a line with the pubis, I carefully cut through the fibres of the glutei and other muſcles; and as the principal arteries and veins appeared, I tied them up ſo as to prevent any conſiderable hæmorrhage, and divided them. The principal nerves leading to the extremities were carefully left untouched, and I proceeded in this manner until no communication was left between the extremities and the ſyſtem at large, except through the medium of the principal nerves, the extremities being left united to the body by theſe and the bones. Between the ſkin and muſcles of the extremities, I next injected a quantity of the watery ſolution of Opium, and laid the animal by. It died in little more than *an hour.*

N° 2. Without any diſſection or diviſion of muſcles, nerves, or blood veſſels, I introduced between the ſkin and muſcles of the poſterior extremities of this frog, a quantity of the ſame ſolution, which killed the

the animal in about the *ſame time* as the former, but not ſooner.

N° 3. Having diſſected this frog in the ſame manner as N° 1. and laid it by, without applying any Opium, I found it lived about *ſeven hours*.

In repeating the experiment upon three large frogs, I found, that thoſe treated as N° 1. and 2. lived near *two hours*; that as N° 3. about *nine*.

This experiment appears to me to prove to a demonſtration, as far as phyſical poſitions admit of ſuch proof, that Opium operates on the nervous ſyſtem, or living principle only; for we ſee by it, that its effects were not even retarded, by all other communication between the parts expoſed to its action and the ſyſtem being deſtroyed: and the many hours which the animals ſurvived the diſſection neceſſarily employed, when they did not ſuffer from the additional power of Opium, clearly proves, that their deaths were occaſioned by the operation of the medicine, and not by the mutilation they underwent.

Such

Such is the view of the queſtion reſpecting the operation of Opium on the fluids; and from the whole I think it appears, that the changes it was ſuppoſed to induce in them, have been rather the children of hypotheſis and fancy, than diſcovered and proved by reaſon and experiment: and we accordingly find, that theſe ſuppoſed alterations always varied with the whim of the writer; ſome aſſerting, appealing to experiment, that it rarefied and rendered fluid, and others that it thickened, coagulated, and congealed the circulating maſs *. To me it appears pretty clear, that any concluſions deduced from experiments made on blood drawn from its veſſels, in proof of its action thereon, are perfectly fallacious and inconcluſive; and that the changes obſerved in it on diſſection, have been rather the conſequences of the previous ſtate of the ſyſtem, the action of the veſſels on their

* Compare Wedelin's Opiolog. p. 145, 158, and the authors he quotes. Friend. Emmenolog. cap. xiv.—— Mead on Poiſons, art. Opium.——Tralles Uſus Opii, vol. i.——Fontana, &c.

contents,

contents, and the effects of expoſure to the atmoſphere, than to any immediate operation of the medicine itſelf on the fluids. I truſt it has at any rate been made ſufficiently apparent, that the ſubſtance in queſtion operates on the nervous ſyſtem, or living principle only; and that it is from conſidering its effects thereon, that we can alone determine its mode of action and operation.

CHAP.

CHAP. VI.

Of the remaining Opinions; and an Attempt to explain its real Mode of Operation.

HAVING referred the operation of Opium to the living ſolids alone, it ſeems neceſſary, before we attempt to inveſtigate its peculiar mode of action, to premiſe a few obſervations on the nature and properties of this part of the animal ſyſtem.

With reſpect to the functions attributed to the brain, and its productions the nerves, the generality of writers have pretty much agreed; in attempting to explain them, no ſmall incongruity of opinion is obſervable. A particular examination, however, of each author's opinions upon this ſubject, ſeems by no means requiſite. So many convin-

cing

cing arguments have been adduced againſt the opinions of thoſe who attributed the nervous functions to tremors and vibrations, that the hypotheſis ſeems at preſent univerſally deſerted. Thoſe who have aſcribed them to the electrical fluid, have not met with many advocates; and their opinions ſeem totally irreconcileable to a few ſimple facts. The equal facility with which this fluid pervades every part of a living ſyſtem, and the total inſenſibility of any member, on the nerves of which a ſlight ligature has been made, ſeem alone ſufficient to refute the idea *.

From the firſt commencement of any controverſy upon this ſubject, the prevailing opinion ſeems to have been, that the immediate agent in all the nervous or animal functions was a peculiar fluid ſecreted into, and freely flowing through the cavities, or inherent in the ſubſtance, of the nervous fibres. This precedence it ſtill retains, and, as the only explanation at preſent offered,

* See alſo Whytt on the Vital and Involuntary Motions, ſect. x.

ſeems

ſeems alone to merit a particular diſcuſſion. To me, I muſt confeſs, it appears no better founded than any other advanced upon the ſubject, and I ſhall briefly ſtate the reaſons which have induced me to form this opinion.

In examining any particular theory, our attention will be naturally turned to the proofs adduced in its ſupport, the explanation it affords of any phænomena in queſtion, and its coincidence with, or repugnance to, particular facts and experiments. If the firſt be deficient or fallacious, the next unſatisfactory, and facts and experiments inexplicable or inconſiſtent occur, the opinions in queſtion are ſurely to be deemed erroneous, and conſigned by all to merited oblivion. I ſhall, in this order, conſider the ſentiments of thoſe who ſupport the exiſtence of the nervous fluid.

That no *proof* either has been, or can be given, of the exiſtence of cavities in the nerves, or of a fluid circulating through or inherent in them, is by all acknowledged; the moſt favourable character, therefore,

which

which can be aſſigned the opinion in queſtion, is that of a probable *hypotheſis* *. The only experiment in ſupport of the opinion worthy of diſcuſſion, which I have met with, is that ſaid to have been firſt made by Bellius, and repeated by Bohn, Pitcairne, and Monro ſen.; in which the motion of the diaphragm in a living dog, which was ſtopped by compreſſing the phrenic nerves, was again renewed by ſtripping the nerve from the fingers, which make the compreſſion towards the diaphragm †. But independently of a very material circumſtance, to wit, that this experiment by no means invariably ſucceeds ‡; it will be evident, that the motion of the diaphragm, ſaid to ariſe from ſtripping the nerve downwards, is the conſequence of the irritation of the nerve, if we conſider, that muſcles will contract either in or out of the body,

* Haller very properly terms his diſquiſitions on this point, *Conjecturæ.* Elementa Phyſiologiæ, lib. x. ſect. 8.

† Monro ſen. Treatiſe on the Nerves.——Pitcairne, Elem. lib. i. c. 5.

‡ Monro jun. Nervous Syſtem.——Halleri Elementa, lib. x. ſect. 8. § 17.

whether the nerve which ſupplies them be ſtripped in a direction towards them, or its oppoſi e, or be merely pierced with the point of the fineſt needle *. The arguments adduced in ſupport of the opinion, ſeem equally, or even more fallacious. The firſt, advanced by Dr. Haller, is founded upon the ſuppoſed ſuperiority of this hypotheſis over that which attributes the nervous functions to tremors and vibrations. After having refuted this laſt, by very ſufficient and convincing arguments, he ſays, "Supereſt ut ſit *probabile*, liquidum eſſe quod a cerebro adveniat, in nervos deſcendat," &c. †; and in the next paragraph, "Cavas ergo eſſe fibrillas nerveas, & quarum eadem natura eſt cerebri medullares fibras, *probabile* fit." But demonſtrating one opinion to be ill-founded, is by no means giving a proof that an-

* This I have myſelf often experienced. See Haller. Primæ Lineæ, ſect. 403.———Element. lib. x. ſect. 7. § 31.

† Ibid. ſect. 377. The mode of reaſoning in his Elementa is ſimilar, lib. x. ſect. 8.

other

other is free from error *. Another argument has been deduced from the great quantity of blood ſent to the brain: the brain, ſay they, receives a very conſiderable proportion of blood; and we know not what end ſuch a diſtribution can anſwer, unleſs it be deſtined for ſecretion; and if a fluid be ſecreted, the nerves muſt be the excretory ducts, and are conſtantly replete with the fluid thus produced. But this is founding an argument upon our own ignorance; nor ſhould we, becauſe we know not the uſes ſuch a quantity of blood may be intended for, conclude, without farther proof, that it is employed in a peculiar ſecretion †.

In

* "This, indeed, is the common refuge of all hypotheſes, that we know no other way in which the phænomena may be produced, and therefore they muſt be produced in this way." Reid, Eſſays on the Intellectual Powers of Man, Eſſay ii. chap. 3.

The whole of the reaſoning in this chapter, though directed principally by the ingenious writer againſt the hypotheſis of nervous vibrations, applies with equal force againſt that of a nervous fluid.

† That life is the conſequence of ſtimulant or exciting powers, variouſly applied to the vital ſolids, and

that

In ſearching then for facts and arguments in ſupport of this hypotheſis, we find them either deficient or fallacious; and the explanations reſulting from it are equally unſatisfactory. Are our ideas with reſpect to the immediate manner in which the motive faculties of the body act, or the ſenſitive are affected, enlarged or elucidated by

that of theſe exciting powers blood is among the moſt conſiderable and conſtant, are poſitions which appear to me ſufficiently evident. See Cullen's Phyſiology, ſect. 136.——Brown's Elementa, ſect. 13. And from them I would be led to conclude, that ſo large a maſs of nervous matter as the brain, requires a conſiderable proportion of blood for its ſufficient excitement, and is, on this account, ſupplied with ſuch capacious veſſels. If it be, moreover, conſidered, that not only the brain receives a large proportion of blood, but that the nerves are univerſally accompanied by numerous blood veſſels; that general debility and ſyncope are often the immediate conſequences of great evacuations of this fluid, or the revulſion of it from the brain occaſioned by tapping; that ſuch conſequences are obviated or removed merely by a horizontal poſture, which prevents too great depletion of the veſſels of the head, and that completely obſtructed circulation in any part is ſpeedily followed by its loſs of life; ſuch an idea will appear, I imagine, at leaſt extremely probable.

ſuppoſing that, in the one inſtance, a fluid moves from the brain; in the other, is moved towards it? Surely not. Our knowledge of the ſtructure and action of the muſcular fibre, or acquaintance with the nature of the ſenſitive faculty of animals, remain equally obſcure, whether the hypotheſis be received or rejected.

That particular facts are either repugnant to the opinions in queſtion, or inexplicable, even if they be admitted, will be evident, I imagine, from the few following conſiderations: One nerve frequently ſends off different branches, ſome deſtined for ſenſation, others for motion *; and the innumerable fibres of which the trunk of every nerve is compoſed, far from remaining diſtinct from their origin to their termination, are ſo repeatedly united to, and ſeparated from each other, that it is impoſſible to trace one void of ſuch connection for any length of way †. This ſtructure is ſurely in-

* Haller. Primæ Lineæ, ſect. 384.——Element, lib. x. ſect. 8. § 22.

† Monro's Nervous Syſtem.

compatible with the opinion of ſenſation being occaſioned by the nervous fluid's moving towards the brain, and muſcular actions depending on its motions from it; both functions are frequently exerciſed together, and we can hardly ſuppoſe that a fluid moves at one and the ſame time through a long, extremely minute, and variouſly inflected cavity, in oppoſite directions, without irregularity or confuſion. If the nerve, which ſupplies any particular limb or muſcle, be divided and preſſed between the fingers from its inſertion in the muſcle towards its divided extremity, the fibres it ſupplies, as I have myſelf often experienced, will be thrown into convulſive actions: but if the action of a muſcle be the conſequence of the nervous fluid's being urged in unuſual quantity into its fibres, we can hardly ſuppoſe that ſuch effects would follow from an experiment, whoſe only tendency, did ſuch a fluid exiſt, would be to move it in an oppoſite direction. Similar convulſive actions will enſue, if the nerve of any muſcular part be irritated by a ſharp and

pene-

penetrating inſtrument. Should we attempt particularly to explain this fact by the hypotheſis under conſideration, it can only be by ſuppoſing, that the nervous fluid is ſet in motion either by ſome power reſiding and acting in the brain, by the contraction of the coats of the nervous canal, or by an inherent power in the fluid itſelf. That the firſt ſuppoſition is erroneous, will be evident, on conſidering that the ſame effects will follow, though the communication between the part irritated and the brain be completely divided. That the nervous coats contract, would be an aſſertion not only repugnant to the total want of motion obſervable in nervous matter, but productive of no ſatisfactory explanation; it would be only explaining muſcular action in one part, by taking for granted its pre-exiſtence in another: and that a ſecreted fluid ſhould poſſeſs in itſelf the power of motion, would be an hypotheſis void of proof or probability, and incompatible with the generally acknowledged properties of matter.

 The

The reſearches of mankind, ever ambitious of penetrating the moſt remote receſſes of nature, have been frequently extended to ſubjects, which either from their own ſubtilty, the natural defects of the human underſtanding, or both conjoined, muſt ever remain to us involved in impenetrable obſcurity. That among ſuch we are to rank the nature of the living principle of animals, appears extremely probable; and it at any rate would ſeem ſufficiently evident, from the preceding ſlight examination of the only hypotheſis which at preſent prevails upon the ſubject, that it has hitherto completely eluded every inquiry, and ſtill continues obſcure. In place, therefore, of perſiſting in ſuch fruitleſs reſearches, or miſapplying the exertions of ingenuity in attempting to ſupport ill-founded hypotheſes, it will ſurely be preferable at once to relinquiſh purſuits ſo unprofitable, and candidly avow our ignorance where it is ſo apparent. And, notwithſtanding this mode of conduct may by ſome be deemed totally

repugnant to the further extenſion of ſcience, maturer conſideration will be ſufficient to ſhew, that it muſt tend rather to advance than retard the acquiſition of genuine and uſeful knowledge. For, although an inſight into the real nature and operation of the cauſes of any important phænomena muſt, if acquired, be followed by the moſt beneficial conſequences, yet where the inefficacy of repeated inquiries, and a more perfect acquaintance with the real powers and capacity of human intellect have ſhewn ſuch attainments impoſſible, the ſole tendency of further perſeverance will be to divert our views from the only remaining paths to knowledge, the acquiſition and generalization of ultimate facts.

Actuated by theſe conſiderations, I ſhall next proceed, inſtead of any further ſimilar diſquiſition, to ſtate ſuch facts, with reſpect to the nature of the living principle of animals, and the action of ſome medicine thereon, as ſeem requiſite in order to aſcertain the immediate operation of Opium; ſubjoining to each the proofs, arguments,

and authorities neceſſary to evince their truth, and ſuch explanations as any new ideas contained in them may ſeem to require.

That living animals are liable to be affected by the application of various foreign powers, has been ever ſelf-evident and univerſally acknowledged; but ſome modern phyſiologiſts ſeem to have been the firſt who, from more extenſive views of the animal œconomy, were led to conclude, that life is merely the reſult of the operation of thoſe powers upon the living principle, that upon their action life is in conſtant dependance, and that among them are to be enumerated not only the various articles of diet and medicine, but the circulating fluids, heat, the paſſions, and other operations of the mind, muſcular exerciſe, &c *. With reſpect to the ſeat of this living principle, or, as it has been very properly termed, *excitability* of the ſyſtem, we only know to a certainty, that it is ſeated in what are

* Cullen's Phyſiology, cxxxvi.——Brown's Elementa, cap. ii.

uſually

usually reckoned the solid parts of the body, and that in the fluids we can find no traces of its existence *: for, although it be evident that the brain and nerves are possessed of a very considerable portion of it, and are, perhaps, in the more perfect animals, its principal seat; yet many observations and arguments would incline us to believe, that the muscular fibre, and many other parts of the living animal, are possessed of excitability independent of the nervous system †.

Of the nature of this principle, whether

* See chap. v.

† I deem it unnecessary to enter more at large into this subject. Any conclusions to be hereafter drawn will stand unaffected, whether we suppose the nerves alone the seat of excitability or the contrary. Whoever desires a more intimate acquaintance with the controversy on this subject, may consult on one side Haller. Elem. Physiologiæ, tom. i. et iv. Opera Minora, tom. i. and his Memoires sur les Parties sensibles et irritables.——— Bonnet, Contemplation de la Nature.———Fontana, Recherche sopra la Fisica Animale. On the other, Whytt's Essay on the Vital and Involuntary Motions. ———Smith, Dissertatio Inauguralis de Actione Musculari.———Cullen's Physiology.———Monro on the Nervous system.

it be material, or merely a quality with which the ſolids are endowed, or of the particular manner in which it is affected by the exciting powers, we are entirely ignorant; and have therefore only to note, and reduce to ſome general heads, the conſequences reſulting from the operation of the different ſubſtances affecting it; and I ſhall therefore proceed to conſider and ſtate ſuch as appear more immediately connected with the ſubject under conſideration.

The vital ſolids then of the body are endowed with a principle termed excitability, in conſequence of which they are affected by the application of various powers, which have been termed *exciting* or *ſtimulant*, although the latter term has been generally applied to ſubſtances employed with a medicinal intention. The general reſult of the combined operation of theſe exciting powers, is what has been termed the *excitement* * of the ſyſtem; on the production of which life depends, and whoſe various de-

* Cullen and Brown as above quoted.

viations

viations from the juſt medium of health give riſe to a variety of phænomena. Of theſe powers many affect the excitability of the ſyſtem in conſequence of its natural formation; ſuch are animal heat, blood, the paſſions, &c. Many are applied in conſequence of deſires implanted in the animal; ſuch are the different articles of diet, external heat, and others; and many, eſpecially in man, are employed, and ſometimes become neceſſary, from the depravity of appetite, or preſence of diſeaſe. That the excitement of the ſyſtem is the conſequence of the operation of theſe powers, will be evident on conſidering that this excitement is increaſed by their being applied in unuſual quantity, is enfeebled by their diminution, and becomes completely extinct, if they be ſubtracted entirely, or to a certain degree; of the truth of which the well-known effects conſequent to an unuſual quantity, great evacuation, or nearly total loſs of blood, or of a generous and ſpare diet, will, if contraſted, be ſufficient proofs.

That the ultimate operation of all exciting powers is ſimilar, and differs only in degree, will be at leaſt rendered very probable, from conſidering, that the application of one is ſufficient to obviate for ſome time the bad effects that would otherwiſe enſue from the ſubtraction or diminution of another. But as ſome ſubſtances, eſpecially of thoſe medicinally employed, are found by experience to poſſeſs ſtimulant properties in a much more eminent degree than others, a peculiar claſs has been formed by writers on the Materia Medica, under the title of Stimulants, to which ſuch ſubſtances are particularly referred. Of this claſs the principal articles are well known; and as, in the ſubſequent part of this chapter, we ſhall have frequent occaſion to recur to and conſider their properties, I ſhall here particularly examine and ſtate ſuch neceſſary facts as we are acquainted with concerning them.

The various ſubſtances ranked under the claſs of Stimulants, however ſimilar in their

 mode

mode of operation in other reſpects, differ materially in two principal points, viz. the different degrees of *permanency* and *diffuſibility* poſſeſſed by each*; the effects of ſome diſappearing very ſhortly, thoſe of others remaining for a much greater length of time: ſome affecting the ſyſtem at large almoſt immediately on their application, while the operation of others is entirely confined to a particular part, or but ſlowly affects the excitability in general. And although it might be *a priori* expected that the diffuſibility or permanency of the effects of any article would be proportioned to its degree of power as a ſtimulant, yet experience ſhews this is by no means the caſe; the effects of ſome very active medicines of this claſs being almoſt entirely confined to the part they are applied to; and the conſequences reſulting from other things, in their nature both powerful and diffuſible, diſappearing much ſooner than the effects

* Brown's Elementa.——Gregory's Conſpectus, ſect. 1161.

of

of others inferior in ſtrength, and tedious in their operation. Particular examples of the truth of theſe aſſertions are ſurely needleſs; a ſlight conſideration of the effects of the principal ſtimulants uſually employed, will be ſufficient to prove them well-founded.

The particular phænomena induced by the action of ſtimulants may be divided into local and general, and differ according to the particular ſtructure of the part to which they are applied, the different ſtate of the ſyſtem at large, and the greater or leſs degree of intenſity in the ſtimulant itſelf. With reſpect to the *local* effects of ſtimulants, they vary, as juſt obſerved, according to the nature of the part affected. If the organ ſtimulated be ſecreting, the fluid it ſecretes is increaſed in quantity, and altered frequently in quality: if the part to which the ſtimulant is applied be ſenſible, and the ſtimulant powerful enough to penetrate the integuments deſtined to moderate the force of external impreſſions, unuſual redneſs, heat, pain, and other ſymptoms of inflammation in different degrees, are the conſe-

quences

quences of its application. The *general* effects of ſtimuli are more varied; if the body on which they act be in a ſtate of perfect health, one very univerſal effect attending their firſt operation is an increaſe of quickneſs and ſtrength in the pulſe; but it is particularly to be noticed, that ſtimulants operating on the body in a ſtate of debility, frequently diminiſh the quickneſs of pulſe, ſo often an attendant on ſuch a ſtate. Of this we have many ſtriking inſtances. In the latter ſtage of contagious fevers, ſo commonly accompanied by a conſiderable degree of debility, the frequency of pulſe is very remarkably lowered by the exhibition of wine. In pulmonary conſumption, the operation of gentle exerciſe is attended with the ſame effect; many other inſtances are not wanting of a ſimilar nature. Nor is it difficult to account for this fact: for as the increaſed quickneſs in the heart's motions ariſes, in ſuch caſes, principally from the debility of the ſyſtem, it will be diminiſhed by ſuch ſtimulants as are beſt calculated to leſſen or remove ſuch a ſtate.

The frequency of pulſe produced by the action of ſtimulants, as above noticed, is often attended or ſucceeded by an increaſe of heat and perſpiration; and, if the ſtimulants employed have been of the diffuſible kind, the variouſly modified ſymptoms of intoxication enſue. Such are the general effects moſt frequently obſervable from the exhibition of theſe medicines; and from conſidering them, it will be manifeſt, that the excitement of the ſyſtem is increaſed during their operation, and that to this increaſe of excitement, either partially or univerſally induced, their effects both local and general are to be aſcribed.

Yet although an increaſe of excitement be the *immediate* conſequence of the application of ſtimulant powers, the *ultimate* effect either of the long continued application of them in moderate quantity, or of their being applied for a ſhort time in unuſual and conſiderable proportion, is diametrically oppoſite; the excitement thus produced being followed, either after a a length of time, as in the firſt inſtance, or

ſuddenly,

ſuddenly, as in the latter, by its languor and decreaſe *. This diminution of excitement has been termed a ſtate of *Collapſe*, but more properly, as more expreſſive of its nature, *indirect debility*; and that ſuch is frequently produced, is ſufficiently evinced by the conſequences at length reſulting from repeated intoxication, the ſuddenly fatal effects ſo frequently obſerved from an unuſual quantity of ſpirituous liquors taken at once, and many other facts of a ſimilar nature. The long continued, or ſudden and violent application of ſtimulants is alſo productive of another effect; and which, though an attendant on indirect debility, frequently, I imagine, exiſts independent of it. I mean a certain degree of *inſenſibility* to the application of ſtimuli, otherwiſe ſufficiently powerful. Many facts concur in proving, that this ſtate may exiſt where indirect debility has not been induced. An inhabitant of this climate, for inſtance,

* Cullen's Phyſiology, and Firſt Lines, paſſim.—— Brown's Elementa, c. 2.———Gregory's Conſpectus.

 though

though in perfect health, if removed to a very northern latitude, will be affected with various complaints, in consequence of not being acted upon by the usual degree of heat, from which the inhabitants of the country, though exposed to the same temperature, are at the time totally exempt; which is merely the consequence of the stimulus of heat having been previously applied in greater proportion to the former than to the latter; by which he has been rendered insensible to the operation of the degree of heat to which he is still exposed, and which is sufficient for the constitutions of those who have not been accustomed to it in greater proportion. If the hand immersed in water of considerable warmth, be taken out and plunged in a vessel containing a quantity of the same fluid of a somewhat lower temperature, instead of the sense of heat which would have arisen were it not for its previous application in a more considerable degree, that of cold will be the consequence. Women, children, and such as have been little accustomed to the use of

ſpirituous liquors, or other ſtimulants, will be ſtrongly affected by ſuch a quantity of them as will produce but trifling conſequences on thoſe who have previouſly employed them in greater quantity, though both be at the time in an equal ſtate of health and vigour. So that it ſeems ſufficiently demonſtrable, that a certain degree of inſenſibility to the action of ſtimulants may be the conſequence of their previous application, though their force or quantity has not been ſo conſiderable as to induce a ſtate of debility or diſeaſe

Theſe few remarks on the nature of the vital ſolids of animals, and the action of ſtimulants thereon, being premiſed, we ſhall next try their application to the ſubject before us, and endeavour to inveſtigate the real mode of operation of Opium in producing the effects which render it ſo remarkable.

As it has been ſufficiently ſhewn that we are entirely ignorant of the real nature of that principle to which the phænomena of life are to be referred, it follows as a

neceſſary conſequence, that every attempt to explain the operation of any medicine whatever, from any ſuppoſed mechanical or other alteration in the qualities of an imaginary fluid, are to be conſidered as purely hypothetical; and we ſhall therefore omit, as unneceſſary, any further diſcuſſion of thoſe theories which have aſcribed its effects to a diminiſhed mobility of the nervous fluid; whether the authors of them have attempted to particulariſe the mechanical manner in which this immobility was ſuppoſed to be induced, or totally neglected ſuch an explanation. If the data be imaginary, the concluſions drawn from them demand not more particular attention. It remains then only to determine, as will appear from the chapter in which I have given a detail of the principal theories on this head, whether the effects of Opium are to be aſcribed to ſtimulant and ſedative powers united in different proportions, to a ſtimulant power alone, or to its diametrically oppoſite, a ſedative. The former opinion I ſhall firſt conſider, and deem

deem it not difficult to ſhew, that its ſupporters have given no proof of its being well-founded, and that a few arguments may be urged againſt it ſufficient to enforce its general rejection.

It is in the firſt place to be remarked, that Opium cannot be ſeparated into any two principles endowed ſolely with the oppoſite qualities of a ſtimulant and ſedative, as will be ſeen by recurring to the chapter in which its analyſis is contained. The principal argument on which thoſe who attribute to this medicine both ſtimulant and ſedative qualities rely, is deduced from its being found firſt to increaſe, and afterwards diminiſh the frequency of the pulſe: but, as will be hereafter ſhewn, this effect can be explained in a much more ſatisfactory manner; ſo that there is no neceſſity of reſorting in this caſe to a ſuppoſition, rendered highly improbable from conſidering, that we know of no ſimple in the whole Materia Medica poſſeſſed of powers in their nature oppoſite, and operating, when it is exhibited, ſeparately and diſtinctly. But even

granting that Opium is endowed with these contrary qualities, it will be difficult to explain on such data the consequences of its exhibition; for as the effects of such principles are supposed to be diametrically opposite, it naturally follows that one must counteract the other, and hence if both exist in equal proportions the medicine would turn out perfectly inert; if one preponderates, it will act solely in consequence of the excess of that principle, and to it alone, whether stimulant or sedative, will all its effects be ascribable.

This question thus briefly discussed, we are next to determine which property Opium is endowed with, that of a stimulant or sedative. The facts and arguments I am about to relate have induced me to conclude, that all its effects are the result of a stimulant property alone; and as the nature of a stimulant and what has been termed a sedative are perfectly opposite, I shall look upon it as a sufficient proof that Opium is not endowed with the latter as a primary quality, if it be clearly ascertained that it

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poſſeſſes the former in conſiderable degree; that all its effects are clearly deducible from this property alone; and that every objection to be urged againſt ſuch explanation can be anſwered in a full and ſatisfactory manner.

Whether Opium is poſſeſſed of ſtimulant properties, can only be determined by conſidering the effects it produces when applied to the living ſyſtem; and that it really does exert ſuch a power upon the body in a ſtate of health, will be evident from the enumeration of its effects in the ſecond chapter. It will there be found, that, applied to the tongue, its taſte is pungent and heating; diſſolved in water, and poured into the eye, it induces pain, heat, inflammation, and increaſed ſecretion from its glands; applied to excoriated and inflamed parts, it proves painful and irritating; dropped on the hearts of animals, it accelerates their motions, or rouſes them into action if they have previouſly ceaſed to vibrate; and taken internally, it quickens and ſtrengthens the contractions of the heart and arteries, increaſes the

the heat of the body in general, augments perſpiration or induces ſweat, excites a diſpoſition to venery, and gives temporary vigour to body and mind. Such are the *primary* effects reſulting from its partial or general operation on the body in a ſtate of health, and ſuch can be the conſequences of a ſtimulant power alone. That it ſhews manifeſt ſigns of the ſame property when operating upon the ſyſtem in a diſeaſed ſtate is equally evident. In the latter ſtage of typhus fever, attended with delirium, ſubſultus tendinum, and other ſymptoms ariſing at that period from the great debility of the ſyſtem, like wine, the volatile aromatic ſpirits, and other ſtimulants either alone or in conjunction with them, Opium has the moſt ſalutary effects. Of this the moſt reſpectable authors and practitioners have deſcribed and witneſſed a variety of inſtances. In intermittent fevers it has frequently prevented the recurrence of a paroxyſm, when given before its expected approach; or even when exhibited after its commencement, it moderates its violence,

lence, and brings it to a ſpeedy and eaſy termination; in theſe effects reſembling the volatile and ammoniacal ſalts, aromatics, and many other ſtimuli, which have ſo frequently been preſcribed with ſimilar intentions and event. In the confluent ſmall-pox, where a weak and quick pulſe, flat and watery puſtules, pallid ſkin, and other ſimilar ſymptoms, denote a conſiderable degree of debility preſent, like wine and other cordials it is ſtrongly indicated, and frequently produces moſt deſirable conſequences; and in a variety of ſpaſmodic affections it is, as well as other ſtimulants, a remedy of acknowledged efficacy. But, deferring to a ſubſequent chapter a more particular enumeration of its ſalutary effects in theſe and ſeveral other diſeaſes, I ſhall content myſelf with referring to the authors already quoted in the ſecond chapter, and tranſcribing from a few others ſome paſſages which will ſufficiently prove, that its ſtimulant properties and cordial effects have been very generally and diſtinctly noticed, and that it has been very frequently and

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ſucceſsfully employed with ſuch intentions by practitioners of the greateſt ſkill and character. And firſt let me place the venerable Sydenham, in general ſagacious in his enquiries, and ever actuated by the ſpirit of fidelity in relating their reſults: engaged in extenſive practice, this medicine was frequently exhibited by him, and in ſo great a variety of inſtances, his attentive mind could ſcarce fail being ſtruck with the ſtimulant powers it ſo obviouſly poſſeſſes; and we accordingly find, that he not only frequently preſcribed it with an intention of ſupporting the powers of nature when languiſhing or oppreſſed, but conſidered it as the moſt ſupreme cordial ever diſcovered: "Et præſtantiſſimum ſit remedium, *cardiacum* unicum pene dixerim *," are the expreſſive words he employs in conveying this ſentiment to his readers. That the celebrated Cullen perceived ſimilar effects, and preſcribed it with ſimilar intentions, will be evident from a ſlight peruſal of his prac-

* Sydenhami Opera, ſect. iv. cap. 3.

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tical works. In Haller's Commentaries on the Inſtitutes of Boerhaave we meet with a paſſage which clearly proves that he alſo was ſtruck with its ſtimulant properties, as he therein compares its action to one of the moſt powerful ſtimuli we are acquainted with. "Opium," ſays he, "non alia ratione agit in corpus, quam alcohol *." A ſentiment alſo adopted by Huxham, who, ſpeaking of the employment of opiates in ſmall-pox, ſays, "They are ſimilar in effect to large doſes of ſpirituous liquors †." Further authorities it would ſeem unneceſſary to quote; yet I cannot forbear noticing two modern publications, in which its cordial properties, and conſequent ſalutary effects in typhus fever, are particularly inſiſted on and confirmed by a variety of practical inſtances. I allude to the writings of Doctors Wall ‡ and

* Vol. iv. p. 519.

† On Fevers, p. 148.

‡ Clinical Obſervations on the uſe of Opium in low Fevers, by M. Wall, M. D.

Campbell,

Campbell *, whoſe obſervations I ſhall have occaſion more particularly to conſider hereafter.

But the ſtimulant qualities of Opium are not only evinced by its ſalutary effects in ſome diſeaſes; its injurious conſequences, if exhibited in others of a contrary nature, tend equally to demonſtrate that it poſſeſſes them in conſiderable degree: for repeated experience has ſhewn, that in the commencement of inflammatory complaints, its exhibition, like that of other ſtimulants, is attended with the aggravation of every ſymptom, and renders the diſorder more dangerous in its progreſs, and difficult in its removal; ſo that if any general rule can be deduced from the practical remarks of ſome of the moſt celebrated names in medicine, one ſurely is, that Opium is to be avoided in every inflammation of the active kind. And although, from the prevailing theories with reſpect to the nature and

* Obſervations on the Typhus, by D. Campbell, M. D.

caufes of inflammation, and the mode of operation of Opium, the former of which has been generally and juftly fuppofed to confift principally in an increafed action of the fanguiferous fyftem, and the latter to operate by diminifhing all action, activity, and fenfibility, we fhould, *a priori*, be led to conclude, that it was a medicine adapted to every fuch complaint, and that fuch conclufion muft have been univerfally acceded to; yet in them its evil tendency has been, to moft practitioners, rendered fo manifeft by experience, that its exhibition has been generally, and with propriety, interdicted. And where, from the confiderable degree of pain which in many inftances accompanies fuch complaints, its ufe has, to a few, feemed advantageous or neceffary, their cautions, with refpect to its employment, fufficiently fhew, that even they have prefcribed it with a timorous hand, and only after repeated evacuations; and that it was more recommended to them by the neceffity of the moment, than by any conviction of its being in the leaft adapted to the removal of

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the proximate cauſe of the complaint. Of theſe aſſertions ſufficient proofs will be found in the writings of the greater number of practical authors, eſpecially of Dr. Cullen *, who is ſtrikingly averſe to the exhibition of Opium in the commencement of any inflammatory complaint; and of Dr. Young †, the reſult of whoſe obſervations on this point is, that it is hurtful in every diſorder, towards the removal of which blood-letting is neceſſary. Pringle ‡, Van Swieten, Boerhaave ‖, and Sydenham §, will be found to concur in the ſame opinion. That thoſe authors who do recommend it in inflammatory complaints, employed it with the greateſt caution, and only after repeated blood-letting, is ſufficiently ſhewn in the writings of De Haen, Huxham, and Storck; but the obſervations

* Firſt Lines, vol. i. ſect. 299, 375, 395, 409; and vol. ii. ſect. 648.———Materia Medica, vol. ii. p. 237.

† On Opium, ſect. 4.

‡ Diſeaſes of the Army, part iii. chap. 1, 2.

‖ Commentaries, aph. 998, &c.

§ Sect. v. cap. 6.

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we ſhall have an opportunity of examining hereafter, when treating of pneumonic inflammation.

Striking, however, as are the various proofs already given of the ſtimulant qualities of Opium, another circumſtance remains to be mentioned equally forcible and convincing, which is, that when cuſtom has rendered its employment abſolutely neceſſary to ſome conſtitutions, if any accident prevents thoſe in ſo unfortunate a ſituation from obtaining a ſufficient ſupply, the bad conſequences reſulting from ſuch deficiency can only be obviated by the frequent and liberal uſe of other powerful ſtimulants. Of this fact a ſtriking inſtance is related by Acoſta, and thus tranſcribed by Dr. Alſton in the Edinburgh Medical Eſſays*: "There were," ſays he, "ſome Turkiſh priſoners and Arabian captives in the ſhip in which I returned from the Indies to Portugal, who had a ſmall quantity of Opium concealed, and uſed it only as a medicine. When they

* Vol. v. p. 1. art. 12. ſect. 3.

had consumed it all, one of them, a Turk of Aden, said to me, Since you have the care of the sick, I must let you know, that unless you give me and my companions Opium, we cannot live two days. I denied I had any; the only remedy then, said the Turk, whereby we who have been accustomed to eat Opium can be recovered, is by a draught of pure wine every morning; though this is very hard and uneasy to us, as being contrary to our law, yet since our health depends upon it, we must submit. By his advice, I gave them all wine; they recovered, and in a month's time would take no more wine, and neither needed nor desired Opium." Prosper Alpinus, during his long residence among the Egyptians, remarked, that many who accustomed themselves to chew Opium constantly, if but deprived of it for a single day, became languid, dejected, and uneasy at the customary hours of taking it, and could only be roused from this state by the usual quantity of Opium, or by a large draught of wine, rendered still more power-

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ful by the addition of pepper and other aromatics: "Animi ſiquidem," ſays he, "deliquio faſtidioſiſſimo tentantur, nulloque auxilio ſic tuto liberantur, quam rurſus Opium devorantes. Multos ab hac ſervitute liberatos vidi, ſi, in hora qua ſoliti ſint ipſum capere, largius ex vino Cretico, pipere, atque aliis aromatibus alterato, potent *."

The various circumſtances I have mentioned, as evincing the exiſtence of a ſtimulant quality in Opium, will even, if conſidered ſingly, be found to contain very ſtrong proof of the truth of the aſſertion; but taken in the aggregate, their force is ſo ſtriking, that I know no medical queſtion on which we can decide with any certainty, if not on the preſent. Notwithſtanding, however, this ſtimulant quality has been, to the generality of writers, ſo very apparent in a variety of inſtances, they will be found, when attempting to explain the mode of operation of the medicine, to proceed upon ſome of thoſe principles we have already

* De Medicinâ Egyptiorum, lib. iv. cap. 1.

endeavoured to refute. Of late, indeed, the ingenious Dr. Cullen has attempted to refer its ſtimulant effects to a ſedative principle, by ſuppoſing, that they are the conſequences of an exertion of the *vis medicatrix naturæ*, or that power which the animal œconomy is ſuppoſed to poſſeſs, of oppoſing every application which has a tendency to hurt it *. But with whatever reſpect we may look up to the practical remarks and precepts of ſo great a name, I fear his theoretical ſpeculations will be frequently found fallacious. In the preſent inſtance, it will be obvious to any perſon who conſults the original work, that this explanation is purely hypothetical, no argument or fact being advanced in its ſupport. After having enu-

* Mat. Med. vol. ii. p. 222, 229.—This explanation of the mode of operation of Opium, is obviouſly very different from that he formerly adopted and ſupported, as will be evident to any who conſult the firſt edition of his Materia Medica, or have heard his Lectures; until the publication of his laſt work, he always ſupported the opinion of the medicine's poſſeſſing a combination of ſtimulant and ſedative powers.

merated and refuted ſome other theories on this point, he proceeds thus: "To explain, therefore, the ſtimulant effects that often appear from the exhibition of narcotics, it ſeems neceſſary to aſſign ſome other cauſe than the direct ſtimulant power of the ſubſtance applied, and it *appears* to be that reſiſtance, &c." But not only is this hypotheſis unſupported by facts or arguments; ſome may be urged againſt it, whoſe force it will be found difficult to elude. In every inſtance where the vis medicatrix naturæ is ſuppoſed to exert itſelf in oppoſing any noxious power operating upon the body, ſome of the diſagreeable effects, reſulting from the operation of ſuch power, are always perceptible before the ſubſequent exertion or re-action, as it has been termed, takes place; but it has been already ſhewn, that the very *firſt* effects reſulting from the exhibition of Opium, are thoſe which ariſe from its ſtimulant qualities; and that the increaſed quickneſs of pulſe, heat of body, and other phænomena of a like nature, are not preceded by languor, diminution of the

velocity or ſtrength of the heart's motions, or any other ſimilar effect; ſo that, if the vis medicatrix naturæ be ſuppoſed in this inſtance to operate, ſhe muſt be allowed to poſſeſs a *previous* knowledge of the properties of the medicine exhibited, and conſequently to exert herſelf, on its firſt application, in obviating their bad effects; a property ſurely as extraordinary as any ever attributed by Stahl or Nichols to their viſionary being, the *anima medica!* Another argument againſt the hypotheſis in queſtion, is deducible from the ſalutary operation of the medicine in ſeveral diſeaſes, wherein nature is weak, languiſhing, and oppreſſed; yet in which its ſtimulant and cordial properties are every day experienced to advantage. If ſuch effects were the reſult of the re-action of nature againſt a noxious power, would it be feaſible or adviſeable to oppreſs her ſtill further, when already languiſhing under a load ſhe is unable to ſupport or oppoſe? This is particularly illuſtrated by one practical inſtance I ſhall ſtate, wherein the Doctor's theory and practice are ſo completely

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at variance, that any further arguments againſt it appear to me unneceſſary. The phænomena of regular gout, he ſuppoſes, ariſe from an atony taking place in certain conſtitutions, which the vis medicatrix naturæ is rouſed to remove, and which ſhe effects by exciting an inflammatory affection in ſome of the extremities; but when the atony has taken place, if nature be ſo weak that the re-action does not ſucceed, the atony continues in the ſtomach and neighbouring parts, and produces the atonic gout, or, as it is commonly termed, gout in the ſtomach *. In this very complaint, however, we find, from a paſſage in his Materia Medica, he has given Opium to the amount of ten grains twice a day with ſucceſs †, a practice ſurely indefenſible, if his theory of its action were founded in truth; for, if nature could not be rouſed into re-action by the cauſe already oppreſſing her, and producing the diſeaſe, with what propriety could a medicine be exhibited to

* Firſt Lines, ſect. 533, 534.

† Vol. ii. p. 262.

aſſiſt her, whoſe exciting powers only ariſe from a ſimilar indirect mode of operation?

As we have ſhewn, then, that Opium poſſeſſes the properties of a ſtimulant in a very eminent degree, and that every attempt made to explain its effects, by attributing them to a ſedative, or any other imaginary quality, has failed; if every phænomenon reſulting from its exhibition can be fairly aſcribed ſolely to the ſtimulant powers it ſo evidently poſſeſſes, where lies the neceſſity of torturing the imagination for any other explanation? And although, as far as I can collect, no ſufficient explanation has been given by any author, of its remarkable pre-eminence over other ſtimuli in allaying pain and ſenſibility, and inducing ſleep and other ſimilar effects, yet they appear to me to be capable of greater elucidation on theſe principles than any other, and I ſhall therefore particularly detail and illuſtrate my ſentiments on this part of the ſubject.

The primary effect of all ſtimulants, as has been already noticed, is to increaſe the excitement

excitement of the ſyſtem; but if the quantity or force of the ſtimulant employed has been ſufficient to raiſe it in any conſiderable degree, this increaſe of excitement is ſucceeded either by a degree of inſenſibility to the further operation of ſtimulants, or by debility of the indirect kind. The firſt effect, then, of Opium exhibited in the uſual doſes, as poſſeſſed of ſtimulant properties in an evident and conſiderable degree, will be to raiſe the excitement of the ſyſtem; which increaſe will be ſpeedily followed by the inſenſibility above noticed: during the continuance of which, all pain will be diminiſhed; all inordinate action, as ariſing either from irritation or increaſed ſenſibility, repreſſed; and watchfulneſs, as generally proceeding from ſimilar cauſes, removed. If the doſe be larger, or the conſtitution little accuſtomed to the uſe of that or other ſtimulants, indirect debility proportioned to the quantity taken, attended with ſtupor, tremor, convulſions, and other ſimilar ſymptoms, will ſucceed the temporary increaſe of vigour, and ſometimes terminate in death

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itſelf. But are we not poſſeſſed, it will naturally be aſked, of other medicines, whoſe ſtimulant powers are far more conſiderable than thoſe of Opium, and yet from the exhibition of which we neither expect nor experience any anodyne effects? And if to its ſtimulus alone theſe effects are to be aſcribed, would they not neceſſarily be poſſeſſed by every ſtimulant in proportion to its intenſity as ſuch? That we are poſſeſſed of articles more conſiderably ſtimulant than Opium, is certain; but that all ſhould prove anodyne in proportion to their degree of ſtimulant power, is neither to be admitted nor expected. We are here to take into conſideration other qualities poſſeſſed by ſtimulants in varied proportion, I mean the different degrees of *diffuſibility* and *durability* with which each is endowed, a difference productive of extreme diverſity in their operation and effects. That any ſtimulant, however powerful, whoſe effects are confined to a particular part, ſhould increaſe or diminiſh the excitement in general by its immediate operation, is not to

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be expected; its only influence on the syſtem at large is in conſequence of the inflammation or other local effect it may produce. A certain degree of diffuſibility in the article employed, is therefore neceſſary to the production of any immediate alteration in the general ſyſtem; and its effects on the excitement will be, cæteris paribus, proportioned to the degree of diffuſibility it poſſeſſes. With reſpect to the diverſity occaſioned by the different degrees of durability in ſtimulants, it appears ſufficiently evident, that thoſe whoſe ſtimulant effects are moſt ſuddenly exhauſted, provided theſe effects have been ſufficiently conſiderable, are followed both more quickly, and to a more conſiderable degree, by the inſenſibility and indirect debility already noticed, than thoſe whoſe ſtimulant effects are more durable, and exhauſted in a more gradual manner. This concluſion is, indeed, the neceſſary conſequence of a well-known and univerſally admitted maxim, viz. that the effects of any impreſſion upon the ſyſtem are not proportioned merely to the degree

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of power in the operating cauſe, but are varied according to the greater or leſs degree of force which thoſe immediately before applied were endowed with; the ſame cauſe which produces conſiderable effects, if ſucceeding feeble impreſſions, being attended with but trifling conſequences, if preceded in its operation by cauſes of equal efficacy: and hence, of two powerful ſtimulants, uniform in point of diffuſibility and force, but whoſe degrees of durability are different, that will leave the ſyſtem moſt inſenſible to the impreſſions of ſucceeding powers, whoſe exciting force is moſt ſuddenly exhauſted; for if that force is in the other diminiſhed *gradually*, its ultimate effects will ſurpaſs in ſo trifling a degree thoſe of the other powers continuing to operate, that their impreſſions will be nearly as much felt, and poſſeſs almoſt as great force, as if no additional ſtimulant had been applied; whereas the exciting powers of the other being *ſuddenly* exhauſted, their ultimate impreſſion will ſo far exceed that of the remaining ſtimulant powers,

6 as

as to leave the ſyſtem for a time inſenſible to their operation.

A few appoſite examples, deduced from analogous facts with reſpect to ſenſation, will ſerve both to elucidate the preceding remarks, and to give additional proof of their being founded in truth. It is well known, that if the eye be expoſed to a conſiderable degree of light, and accuſtomed, for a ſhort time, to contemplate objects therein, it becomes totally inſenſible to the cuſtomary impreſſions, if *ſuddenly* removed to a darker ſituation; but if this change of ſituation had been *gradually* made, it would ſtill remain ſenſible of thoſe objects, which the ſudden alteration had, in the former inſtance, rendered for a time imperceptible. A man, for example, expoſed to very bright ſunſhine, will become for a time totally blind, if carried into a room in which no greater degree of light exiſts than that uſually termed twilight; but if the brightneſs of ſunſhine be reduced in the uſual gradual manner to a ſimilar ſtandard, his eyes, little affected by ſuch a change, will

clearly

clearly difcern every furrounding object. Or, to give another inftance: if the whole body, or any particular part of it, be expofed to a high degree of heat, 100° let us fay of Fahrenheit's thermometer, and this temperature be fuddenly reduced to 80°, the perfon, on whom the experiment is made, will be ftruck with a fenfation of cold, or, in other words, be infenfible of the heat which actually furrounds him; but if this diminution of temperature had been gradually induced, he would ftill feel warm, or the fyftem would now be fenfible of a degree of heat which the fudden fubduction of the ftimulus had, in the former inftance, rendered for a time imperceptible.

The application of thefe remarks to the fubject before us, is, by this time, I hope, fufficiently evident; and from confidering the whole of them, it appears very juftifiable to conclude, that that ftimulant will be productive of the moft confiderable anodyne effects, which conjunctly poffeffes the greateft degree of ftimulant power, the moft ready diffufibility, and which is, at the fame time,

time, the moſt ſuddenly exhauſted. The whole of theſe properties are accordingly diſcoverable in Opium, to a conſiderable degree. Of its highly ſtimulant power, repeated and ſtriking inſtances have been already adduced: the quick diffuſibility of its effects muſt be obvious to the moſt inattentive, and has been univerſally acknowledged; and that its ſtimulant powers are very quickly exhauſted, appears ſufficiently evident from the increaſe occaſioned by it in the quickneſs of the pulſe in a healthy ſyſtem being ſo ſpeedily followed by an oppoſite effect *, and from its being neceſſary, when we wiſh to derive any benefits from its cordial properties, to repeat the doſe more frequently than that of other ſtimuli ſometimes employed in its place, as will appear more particularly from different paſſages of the enſuing chapter.

To render my ſentiments upon this ſubject as perſpicuous as poſſible, I ſhall next endeavour to bring them under one clear

* See Experiments, chap. ii.

point of view, by reducing the whole of the conclusions to be deduced from the preceding facts and arguments, to a few general heads; and the following appear to me to comprehend them all:

1. Opium is a medicine endowed with a ſtimulant property, conſiderable in degree, readily diffuſible over the whole ſyſtem, and eaſily and ſuddenly exhauſted.

2. In conſequence of the immediate operation of this ſtimulant power, the pulſations of the heart and arteries are rendered quicker and ſtronger, the heat of the body is augmented, its perſpiration promoted, ſweating ſometimes excited, the mind exhilarated, the delirium of intoxication (if the medicine be taken in ſufficient quantity) induced, ſuch morbid ſymptoms as ariſe from general debility removed, and the excitement, in ſhort, of the whole ſyſtem increaſed.

3. But this ſtimulant power being ſuddenly exhauſted, the ſyſtem at large is left inſenſible, in a great degree, to the operation of every exciting cauſe; and as thoſe

continuing

continuing to act are generally inferior in point of exciting force to that which the system was so suddenly deprived of, their effects are rendered less considerable than before its operation: hence the pulsations of the heart and arteries become slower; pain and inordinate action, as arising either from a morbidly acute degree of sensibility, or the presence of irritating powers, are alleviated or totally removed; watchfulness, as produced by similar causes, prevented or suppressed; the mind is rendered dull and languid; and, if the dose has been considerable, indirect debility succeeds; stupor, tremors, and other similar symptoms ensue; the pulse intermits, and death perhaps concludes the scene.

With respect to the objections which may be made to this explanation of the properties of Opium, the only one which can, as appears to me, be urged against it, and which has not been answered, is that founded on an uncontrovertible fact, viz. that Opium applied to different parts of living animals, is *immediately* productive of

total inſenſibility, immobility, and loſs of life, although no ſymptom of increaſed excitement precedes theſe effects. It has thence been concluded, that the medicine operates in every inſtance by a directly ſedative quality: and as it is the principal argument relied on in attempting to prove this opinion, and may be adduced againſt thoſe in this chapter adopted, I have conſidered it with particular attention, but find it not poſſeſſed of any force, nor in the leaſt degree concluſive; and for one ſimple, but very ſufficient reaſon, which is, that many other articles, univerſally acknowledged to be endowed ſolely with a ſtimulant property, are productive, in ſimilar circumſtances, of thoſe effects which, when induced by Opium, are reckoned the conſequences of a directly ſedative quality alone. A few of the experiments I made with a view of determining this point, it may not be amiſs to ſtate; they were indeed among the firſt I inſtituted on the ſubject, at a time when I was engaged in conſidering the validity of the uſual arguments advanced in ſupport of

the generally received opinion with reſpect to the ſedative qualities of the medicine. The ſtimulants I employed were, rectified ſpirit of wine, the volatile alkali, and electricity, three of the moſt powerful we are acquainted with; and my intentions at the time were to determine whether, when applied to animals in a ſimilar manner with Opium, they were not equally efficacious in deſtroying all activity and ſenſibility.

Experiment XXXII.

Having ſeparated by inflation the ſkin and muſcles of one of the poſterior extremities of a frog, I poured between them ſome of a ſtrong watery ſolution of Opium. The limb was deprived of all ſenſibility and power of motion in *ten* minutes.

Experiment XXXIII.

Between the ſkin and muſcles of the poſterior extremity of another, I poured a quantity of ſpirit of hartſhorn, which was productive of the ſame effects as Opium in the former experiment, in about *one* minute.

Experiment XXXIV.

Employing rectified ſpirit of wine in the ſame manner as the other articles in the two preceding experiments, I found it ſucceeded, by ſimilar conſequences, in *three* minutes.

Experiment XXXV.

A ſmart ſhock of electricity having been paſſed through the hind leg of another frog, it *inſtantly* rendered it completely paralytic.

Experiment XXXVI.

Having taken out the heart of a frog, and immerſed it in ſome of the ſame ſolution of Opium employed in Experiment XXXII. I found it deprive it of all power of contraction in *ten* minutes.

Experiment XXXVII.

The heart of another immerſed in ſpirit of hartſhorn, was reduced to a ſimilar ſtate in *two* minutes.

Expe-

Experiment XXXVIII.

Rectified ſpirit of wine deprived a third of all power of motion in *three* minutes.

Experiment XXXIX.

Having perforated a piece of common pane glaſs with a ſmall hole, I laid over it the heart of a frog, and through it ſent a ſmart ſhock of electricity, which inſtantly deprived it of its contractile powers. I took the precaution of uſing the perforated glaſs, as I found the electric fluid was otherwiſe liable to glide over the ſurface of the heart, probably from its moiſture, without affecting it in any conſiderable degree. I muſt obſerve, however, that the ſhocks I employed were not ſo vehement as to injure the organic ſtructure of the parts on which theſe experiments were made.

The preceding experiments, which I have frequently repeated, but with ſo little variation that a particular account ſeems not requiſite, ſufficiently ſhew, that three of the moſt powerful ſtimuli we are acquainted

 with,

with, are capable of producing thoſe effects on the parts to which they are immediately applied, which, when induced by Opium, were deemed the conſequences of its ſedative power, nay, that they are productive of theſe effects even more ſpeedily than Opium itſelf; and it conſequently appears, that any arguments deduced from theſe or ſimilar facts, with an intention of proving the exiſtence of that ſedative quality, are inconcluſive. With a view of comparing the effects of theſe ſtimuli upon the heart, the following experiments were made.

Experiment XL.

Having extended the limbs and tied down a large frog upon its back, I opened the thorax juſt ſo as to bring the heart into view; its pulſations were 62 in a minute. I then injected into the anus, by means of a ſmall ſyringe, ſome rectified ſpirit of wine, but the quantity of which could not be exactly determined, as ſome of what had been injected was again diſcharged. Leaving the animal

animal in this ſituation, I attended to the changes induced in the heart's motions; and reckoning its pulſes at the periods mentioned in the firſt line of the following table, found their number to be as is ſet down in the ſecond:

In	2	5	10	15	20	25	30	35	40	45	50	55	60	minutes,
The P. was	62	62	62	52	46	44	42	40	38	36	34	32	32	

Sixty minutes elapſed, the animal was in external appearance dead, although the heart ſtill continued to beat, and was thrown out.

Experiment XLI.

Having tied down a frog as in the former Experiment, brought its heart into view, whoſe pulſe was 50 in a minute, and injected ſome ſpirits of hartſhorn, I found it affected in the following manner:

In	5	10	15	20	25	30	minutes,
The P. was	50	30	22	6	5	3	

Experiment XLII.

Into a third, whose pulse was 58 in a minute, I injected some of a watery solution of Opium, and found it affect the pulse in the following order:

In	2	5	10	15	20	25	35	40	45	50	55	60	minutes,
The P. was	56	50	4[illegible]	38	37	35	34	32	3[illegible]	32	32	32	

The animal was still possessed of some power of motion and sensibility, but in so trifling a degree that he was killed and thrown out.

Experiment XLIII.

Having tied down a frog and brought the heart into view, whose pulsations were 70 in a minute, I passed a smart shock of electricity through the whole body, from its head to its lower extremities. The pulsations of the heart instantly ceased, and it contracted but once when irritated with a needle. Leaving the animal in that situation, I observed that in about five minutes the

the heart began again to beat, firſt ſlowly, but with increaſing celerity till its pulſations roſe to 35 in a minute. As they did not increaſe above this number, I gave another ſhock ſimilar to the firſt, which was immediately productive of like effects; and after eight minutes more had elapſed, I could not perceive the ſmalleſt ſign of returning pulſation: I then began to draw the gentleſt ſparks from the heart, and was ſurpriſed to perceive it again begin to move, and its contractions, by continuing this application, increaſed gradually till they amounted to 31 in a minute; when, giving the animal a third ſhock, it entirely deprived it of life, for no further application of any kind could rouſe its heart into the ſmalleſt motion.

From the firſt experiments related in this chapter it appears, that ſpirit of wine, and the volatile alkali, deſtroy, even more ſpeedily than Opium, the mobility and ſenſibility of the parts to which they are immediately applied. The ſucceeding experiments

ments ſhew, that although Opium, from its more ready diffuſibility, affects the pulſe *ſooner* than either of them *, yet that they at length are productive of ſimilar effects, if applied in conſiderable quantity. And that the moſt powerful ſtimuli, if applied to the human frame in immoderate doſes, are, as well as Opium, capable of inducing ſtupor, inſenſibility, ſlowneſs of pulſe, and other ſimilar affections, is evident from many caſes in which an immoderate quantity of ſpirituous liquors have been taken at once, and which have frequently terminated in death, preceded by thoſe ſymptoms which an over-doſe of Opium is generally productive of.

The only other fact I ſhall conſider, as demonſtrating that the ſame ſubſtance which in a moderate quantity is capable of exciting the living principle of animals, will, if exhibited in an over-proportion, totally deſtroy their excitability, is the well-known

* Compare Experiments 40, 41, 42.

property

property which electricity poſſeſſes, of inducing paralytic affections if applied in extremely violent ſhocks, and of removing the very ſame complaints if employed in more moderate proportion. This is particularly illuſtrated by a fact noticed by the Abbé Fontana, and further confirmed by the experiment above related. "Electricity," ſays he, "induces death by deſtroying the irritability of the heart and muſcular fibres; and yet this ſame electricity proves one of the moſt powerful ſtimuli we are acquainted with, when applied to any muſcular part. It reſtores life to animals, by exciting that irritability which it had deſtroyed in them the moment before. Of all the ſtimuli which can be employed for recovering thoſe animals which the electric ſhock has thrown into a ſtate of inſenſibility, the immediate application of *gentle* ſparks has appeared to me the moſt efficacious *."

Having

* L' electricité donne la mort, en otant l'irritabilité au cœur, et à la fibre charnue; et neanmoins cette même electricité

Having endeavoured to obviate ſuch objections as may be urged againſt the explanation ſupported in the preceding pages, it may be deemed ſuperfluous to attempt a further vindication of ſuch an enquiry. But it has been by ſome remarked, that any inveſtigation of the mode of action of medicines uſually termed ſedative is unneceſſary; for that as they are only exhibited with a view to their ultimate effect, it matters not in what manner theſe effects are brought about, or whether it be determined that they are the conſequences of a preceding ſtimulant exertion or not.

Such I ſhall anſwer in the words of the immortal Bacon, by whom mankind, firſt

electricité eſt un des plus forts ſtimulans qu'on connoiſſe pour la fibre muſculaire; elle rend la vie en excitant l'irritabilité à ces mêmes animaux, dans leſquelles elle l'avoit detruite, un inſtant auparavant; parmis tous les ſtimulus qu'on peut employer, pour rappeller à la vie les animaux que la commotion electrique a fait tomber en aſphyxie, les etincelles *legeres*, appliqués apropos, m'ont paru le remede le plus efficace."

Traité ſur le Venin de la Vipere, &c. tom. i. p. 78.

extricated

extricated from the labyrinths of imagination, were conducted to the direct and only true paths to knowledge, and whose injunctions should be strictly followed in every scientific research. "Hoc ipsum," says he, "*prius* et *posterius* in omni actione naturali notare debet *." A caution to be especially kept in view where the primary action of a medicine is so obvious and important as that of Opium; and the neglect of which is so strikingly censured, and its observance in every instance so strongly recommended by the same author in the following words: "Mira est hominum circa hanc rem indiligentia; contemplantur siquidem naturam tantummodo desultoriè, & per periodos, & postquam corpora fuerint absoluta, & completa, & non in operatione sua. Quod si artificis alicujus ingenia & industriam explorare & contemplari quis cuperet, is non tantum materias rudes artis, atque deinde opera perfecta conspicere de-

* Novum Organum, lib. ii. aph. 46. circa finem.

siderat,

ſiderat, ſed potius præſens eſſe, cum artifex operatur, & opus ſuum promovet: atque ſimile quiddam circa naturam faciendum eſt *."

* Ib. aph. 41.

CHAP.

CHAP. VII.

On the Pharmaceutical Treatment of Opium, and its Uſe or Abuſe in particular Diſeaſes.

IN this chapter I ſhall offer a few remarks upon the employment of Opium in thoſe diſeaſes, towards the removal or alleviation of which it has been generally recommended, and truſt that the explanation given in the preceding pages, of its mode of action, will aſſiſt in directing us when and how to exhibit it, will elucidate the effects reſulting from it in different inſtances, and be itſelf further confirmed by a variety of practical obſervations.

Previous, however, to our entering upon the conſideration of any particular diſeaſe, a few remarks upon the preparations, doſes,

and modes of exhibiting it, ſeem requiſite; and we ſhall diſpatch the ſubject as briefly as poſſible.

In both the Edinburgh and London Pharmacopœias, we find directions given for the *purification* of Opium from the fæculencies, which all ſpecimens of it contain. But, as it has been ſhewn that its virtues reſide in a principle capable of being volatilized in the heat of boiling water, and as the proportion of inactive fæculencies varies very little in the different parcels brought to market, I confeſs I ſee no neceſſity for any ſuch preparation: ſhould it, however, be deemed adviſeable and neceſſary, that proceſs is to be preferred which will occaſion the leaſt poſſible diſſipation of the volatile parts of the medicine, or that which leaſt requires the application and continuation of heat. Hence the mode recommended by the London college, in the late reformation of their pharmacopœia, would appear the moſt objectionable of any yet deviſed. They direct the Opium to be diſſolved in twelve times its weight of proof ſpirit, which is after-

wards to be filtered, and the ſtrained liquor evaporated to a proper conſiſtence; but, from the long continued heat neceſſary to diſſipate the quantity of menſtruum here recommended, a conſiderable proportion of its more valuable parts will be diſperſed, and the extract be liable, from a greater or leſs degree of heat being employed in the operation, to a variation in point of ſtrength; and we accordingly find, that two grains of this preparation are neceſſary to produce the effect of one grain of the Opium in its unpurified ſtate *. The proceſs recommended in the former edition of their Pharmacopœia ſeems far preferable, in which they order the Opium to be diſſolved in an *equal* weight of boiling water, and, while it remains hot, to be preſſed through a linen cloth, by which means its impurities are left behind, and the ſtrained Opium is then, by the heat of a water-bath, reduced to its original conſiſtence. This proceſs renders the Opium ſufficiently free from impurities;

* Cullen, Materia Medica, vol. ii. p. 258.

and, as the quantity of menſtruum employed is ſmaller, is not near ſo liable to objection as the former. It may, however, I ſhould imagine, be ſtill farther improved, by employing, as a menſtruum, proof ſpirit in place of water, and in the ſame proportion; which would both ſerve to keep the gummy and reſinous parts more uniformly ſuſpended, and require a heat for its diſſipation leſs injurious to the medicine than the ſame quantity of water would; as we have ſhewn above, that its volatile parts do not riſe with the heat neceſſary to the evaporation of ſpirit of wine.

As Opium given in ſubſtance is ſlower in its operation than when diſſolved in proper menſtrua, and as it is ſometimes requiſite that its effects ſhould ſucceed its exhibition as ſoon as poſſible, *tinctures* of it are very neceſſary preparations, and have been recommended with a variety of additions: but ſimplicity ſhould be the leading characteriſtic of every officinal compoſition; and the additions recommended can, if deemed neceſſary, be eaſily joined with

it in extemporaneous prefcription. The *Tinctura Thebaica*, therefore, of the Edinburgh, and *Tinctura Opii* of the London Pharmacopœias, appear the beft contrived compofitions of this nature. Of the two, that of Edinburgh would feem to me preferable, not from the addition of the cinnamon, which may as well be omitted, but from their employing crude Opium in place of the purified, to the preparation of which we have already objected. And as we have feen, from very good authority, that the purified Opium, employed by the London college, is not above half as ftrong as the crude employed by the Edinburgh, it is but fair to conclude, that the ftrength of their tinctures differs in the fame proportion, the quantity of menftruum employed by both being nearly the fame. Of this, however, I have had no real experience.

The *Elixir paregoricum* (E), or, as it is more properly termed *Tinctura Opii camphorata* (L), owes its principal virtues to the Opium it contains; how far the other ingredients prove ferviceable in the quan-

tity prefcribed, appearing very doubtful. It has been principally recommended as an opiate in pectoral complaints.

In the form of electuary, Opium has been prefcribed with a variety of additions. The ancients, ever obfequious to the authority of Galen and his commentators, who deemed the exhibition of Opium dangerous, unlefs accompanied by warm and ftimulating aromatics, and impofed on by the extravagant notions of the age, with refpect to poifons and their antidotes, were folicitous of crowding together every article which experience, fuperftition, or hypothetical reafoning had recommended as corrective of its real or imaginary deleterious properties; and vainly expected, that fo heterogeneous a compofition muft be neceffarily endowed with every virtue which each article was feparately fuppofed to poffefs. Hence arofe that wild exuberance, fo obfervable in thofe electuaries, which, for fo many centuries, held a place in our pharmacopœias. Theriaca, Mithridate, Philonium, and other compofitions of a fimilar nature, which, although

invented

invented before the time of Galen, ſeem to have derived the high character, in which they were for many ages held, to his authority, and that of his commentators. The general properties of theſe medicines were, however, merely thoſe to be expected from the diffuſible and ſpeedily exhauſted ſtimulus of Opium, united with the more durable poſſeſſed by the aromatics, and other ingredients of any efficacy; whence, although they were more effectual than ſimple opiates, when it was found neceſſary to excite and ſupport the powers of the ſyſtem, they were inferior to them in point of virtue, where the anodyne effects of the medicine were requiſite. As a ſtimulant compoſition, however, of equal or ſuperior powers can be eaſily obtained, without having recourſe to ſuch a medley of materials, the ſpirit of reformation has at length juſtly expelled them from both our pharmacopœias; ſo that further remarks on them ſeem unneceſſary. Their places are ſupplied by the

Confectio opiata (L), and *Electuarium thebaicum* (E), two medicines which, though

differing in the proportion of Opium they contain, are, in their general virtues, ſimilar, and are ſometimes ſerviceable in the low ſtages of fevers, ſmall pox, and other diſeaſes to be hereafter mentioned. Thirty-ſix grains of the London, and one hundred of the Edinburgh compoſition, contain one grain of Opium.

Opium is alſo directed to be kept in the form of powder, with various additions. The following are thoſe of the London and Edinburgh pharmacopœias.

Pulvis e creta compoſitus cum Opio (L). This is compoſed of a well-contrived abſorbent powder, conjoined with pure Opium. A prevailing theory formerly, with reſpect to continued fevers, was, that they aroſe from an exceſs of acidity; hence abſorbents were joined not only with Opium, but other warm medicines, as the Contrayerva, &c. employed in their removal: but both this theory, and the practice founded on it, are now exploded; ſo that this compoſition ſeems ſcarcely requiſite. It may, however, be of ſervice in diarrhæa attended

with

with acidity; and the abſorbent powder may ſerve more intimately to divide the Opium: forty-three grains contain one of Opium.

Pulvis opiatus (L). This is merely compoſed of Opium and calcined hartſhorn: ten grains contain one of Opium. The only end the hartſhorn can anſwer, is to divide the Opium, the earth it is compoſed of poſſeſſing very little abſorbent power, being nearly ſaturated with phoſphoric acid.

Pulvis Ipecacuanæ compoſitus (L).

Pulvis Sudorificus ſive Doveri (E).

Theſe, though differing in name, are compoſitions almoſt perfectly ſimilar, and of conſiderable efficacy, being very certain ſudorifics, and, as ſuch, employed in rheumatiſm, in which they were firſt recommended by Dr. Dover, and in ſeveral other diſeaſes: ten grains of the London, and eleven of the Edinburgh, contain one of Opium. It is beſt given in doſes of ten or fifteen grains, which may be repeated every two hours till the ſweat breaks out. In doſes of twenty or thirty grains, which are

ſometimes given, it is apt to excite vomiting.

The opiate pills, at preſent ordered by the two colleges, are, the

Pilulæ Thebaicæ (E), *Pilulæ ex Opio* (L). The latter compoſition ſeems preferable to the former, as more ſimple, and equally ſoluble; it is compoſed merely of Opium, with the extract of liquorice, which, ſome experiments have ſhewn, promotes its ſolution in the ſtomach; to which, in the Edinburgh preſcription, are joined a ſmall quantity of ſoap and Jamaica pepper. Five grains of the London formula, and eight of the Edinburgh, contain one of Opium.

With reſpect to the *ſyrups* obtained from the poppy by decoction, I could wiſh them omitted altogether, as the greateſt care taken in their preparation cannot prevent their being liable to uncertainty and variety in ſtrength. The Tinctura Thebaica, added to ſimple ſyrup in neceſſary proportion, will anſwer every end they can poſſibly be deſigned to effect, without being liable to any ſimilar or other objection.

As to the doſe of Opium, it is impoſſible to lay down any certain general rule, its effects varying according to the conſtitution of the patient, his ſituation at the time of taking it, and his being more or leſs accuſtomed to its uſe. It is generally given to the unaccuſtomed to the amount of a grain. Of the tinctures properly prepared, from twenty to thirty drops are the uſual doſes. Theſe doſes, however, it generally becomes neceſſary to augment, and ſometimes to very conſiderable quantities.

In the exhibition of Opium, we ſhould ever keep in view, not only the conſtitution and habit of the patient, but the intention with which the medicine is at the time preſcribed; a caution which has, I fear, been too much neglected, and which has led many to imagine, that it was only productive of temporary relief in caſes where, if properly managed, it may tend more than any other medicine to the complete removal of the diſeaſe; for, however conſiderable the variety of thoſe diſeaſes be, in which

Opium

Opium has been recommended, it muſt be exhibited in all, either as a ſtimulant or anodyne. The mode of its exhibition, however, ſhould differ materially, when given with one of theſe intentions, from that to be purſued when the other is to be kept in view; for, as its exciting power, though diffuſible and conſiderable, is ſpeedily exhauſted, it ſhould not, when preſcribed with a view to its ſtimulant effects, be given, as the other more durable ſtimuli ſometimes are, in large doſes, and at conſiderable intervals, but in moderate quantities frequently repeated, at the interval of a few hours, as the ſymptoms of the complaint may require, and conjoined with a proportionable quantity of other ſtimuli more durable in their effects. The precautions, however, ariſing from theſe conſiderations, ſeem very frequently neglected. How often is the phyſician ſatisfied with preſcribing his *hauſtus anodynus horâ ſomni*, in caſes which require their reiterated exhibition in the courſe of twenty-four hours, and in which the tranſient relief procured

procured by one draught, would be rendered permanent by the ſame medicine more frequently repeated.

In ſuch complaints, however, as are accompanied by urgent pain, irritation, or watchfulneſs, and Opium is given with an intention of mitigating or removing them, a contrary plan is to be purſued, and the medicine given in a pretty conſiderable doſe; which need not be repeated for a much greater length of time than in the former inſtance; by which means the ſyſtem will be beſt thrown into that ſtate of inſenſibility, on the production of which its anodyne effects depend. The additions made to opiate draughts, are uſually of a ſtimulant nature, and are ſometimes requiſite to make the medicine remain upon the ſtomach. No more than what is neceſſary for this purpoſe, ſhould, however, in theſe inſtances be preſcribed; as, by the greater durability of their ſtimulant effects, ſuch may prevent the medicine from proving anodyne to ſo great a degree as if given by itſelf. Particular rules I avoid, both

with refpect to the dofes, and intervals between them; they vary fo much in different cafes, that the practitioner's own judgment, guided by the effects preceding dofes may have produced, and the more quick or flow return of urgent fymptoms, muft ufually direct him.

Thefe general obfervations premifed, we fhall proceed to confider the employment of Opium in particular difeafes, beginning with

CONTINUED FEVERS.

This very generally prevailing form of difeafe, has by many fyftematic and practical authors been divided into a variety of genera; all of which, however, Dr. Cullen has in his Nofology reduced to three, the Synocha, Typhus, and Synochus; though inclined to doubt whether the two laft are really different difeafes, and rather difpofed to believe that the Synochus arifes from the fame caufes as the typhus, and is therefore only a variety of it. Had he gone further, and laid down all the different forms of continued

continued fever, merely as ſpecies and varieties of the ſame diſeaſe, he would perhaps have approached ſtill nearer the truth; for ſeveral facts and arguments would lead us to conclude, that they only differ in the ſymptoms which attend them, being in different periods of the complaint more or leſs vehement, and variouſly modified. The principal foundation for any diſtinction between theſe fevers, is the different degree of continuance, and greater or leſs vehemence of thoſe inflammatory ſymptoms which uſher in almoſt every ſpecies of fever, and of the ſymptoms of debility which generally ſucceed them. This is evidently the caſe with reſpect to the Synochus and Typhus; the inflammatory ſymptoms of the former being ſucceeded by thoſe of debility which characterize the Typhus, and which are in this laſt diſorder preceded by ſimilar, though ſlighter ſymptoms of inflammation. But this different modification of ſymptoms would ſeem to depend ſolely upon the different conſtitution of the patient affected, the mode of

treatment

treatment purfued, and other accidental circumftances: for the contagion arifing from the fame fource will, in the fanguine and plethoric, produce a fynochus, in thofe of a contrary habit a typhus; and in the cure of fynochus, if evacuations are pufhed too far, they will foon convert it into a typhus; of which again the fymptoms will be rendered very inflammatory, if ftimulating medicines be in the beginning too liberally employed. Whether thefe arguments may be extended to the fynocha, or pure inflammatory fever, fo rarely met with, may be queftioned; but when it is confidered, that this alfo is in the end attended by fymptoms of debility, and that it has been fometimes known to arife from the fame contagion with the others, we may reafonably conclude it belongs to the fame genus.

Almoft every continued fever, then, commences with inflammatory fymptoms, in fome flight and tranfient, in others more confiderable and permanent, and as, in all, fymptoms of debility fooner or later fucceed,

each may, with propriety, be divided into two ſtages, the firſt of inflammation, the ſecond of debility. With reſpect to the inflammatory ſymptoms with which continued fevers uſually commence, it may be further obſerved, that they are in general far leſs violent and durable at preſent, than we would be led to ſuppoſe they were ſome years ſince, from conſulting the obſervations and practice of Sydenham and ſome ſucceeding authors, and comparing them with the practice and writings of many moderns. Dr. Clark of Newcaſtle, in his excellent Treatiſe on Fevers, tells us, that in the courſe of an extenſive practice, he has ſeen but two * inſtances which ſeemed to require the uſe of the lancet; and his cautions, with reſpect to blood-letting, ſeem fully juſtified by the ſucceſs of thoſe caſes he has given to the public. The preſent Dr. Gregory of Edinburgh, whoſe abilities and extenſive and ſucceſsful practice in the Edinburgh infirmary muſt enſure his opinions

* On Fevers, p. 24.

reſpect, is nearly of the ſame way of thinking. His ſentiments with reſpect to blood-letting, the chief means of reducing inflammatory ſymptoms in every complaint, which I take the liberty of copying from ſome notes taken at his Lectures, are as follows: "Blood-letting may perhaps be occaſionally neceſſary in continued fevers, when the inflammatory ſymptoms run very high; but I am more and more convinced every day of its general inutility. Such ſymptoms are very ſoon ſucceeded by great debility, which this evacuation always renders more dangerous. I have ſometimes been led to employ it from the apparent urgency of theſe ſymptoms, but had often occaſion to regret it afterwards. We are ſometimes, indeed, reduced to a dilemma by their threatening immediate danger, while the dread of enſuing debility prevents our employing the remedy; and theſe fears are frequently well-founded, eſpecially if the evacuation be large, ſo that I ſcarce ever employ it. A complete revolution ſeems, indeed, to have taken place in this reſpect,

eſpecially

eſpecially among practitioners here. A candid old gentleman confeſſed to me, that he does not employ the remedy now above once for thirty times he did formerly." To theſe authorities I will venture to add, that I have myſelf never ſeen a caſe of pure fever, but one, which ſeemed to require general blood-letting; and that was in a very robuſt, young, and plethoric country man.

Whoever compares theſe obſervations and practical cautions with thoſe of Sydenham and others, will perceive a ſtriking contraſt; which may, perhaps, be owing both to the conſtitutions of the generality of mankind, being at the time ſomewhat more vigorous than at preſent, and ſtill more to Sydenham's being too far biaſſed by his averſion and oppoſition to the hot regimen of his predeceſſors and cotemporaries, and his high opinion of a contrary plan: we frequently run into one extreme, in our endeavours to avoid the other. I have premiſed the preceding obſervations, as it appears to me, that the exhibition of Opium is improper during the continuance of the inflamma-

tory ſtage of fevers, however the ſymptoms may ſeem to require it; and is, on the contrary, frequently productive of the beſt effects, after the uſual debility has ſupervened. It is not, however, as eaſy as may be *a priori* imagined, to determine in practice when the inflammatory ſymptoms ceaſe, and thoſe of debility commence. The pulſe is in both ſtages preternaturally frequent, but moſt ſo in the latter; in both is the heat of the body increaſed, the thirſt urgent, and tongue foul. The commencement or preſence of debility will, I think, be beſt diſcovered, by attending to the degree of ſtrength and frequency of the pulſe *conjunctly*, and the ſtate of the mental faculties. At the commencement of the generality of continued fevers, the pulſe will be found from 90 to 100, and ſtronger than in health, though in this great variety is obſervable; as ſoon, however, as it begins to increaſe in frequency, and decreaſe in ſtrength, eſpecially if ſymptoms of approaching or preſent delirium appear, it is to be concluded, that the debility, which

gives

gives a dangerous tendency to the diſeaſe, has ſupervened, and that opiates, where required, may be exhibited without danger.

Having ſaid thus much with reſpect to the ſtage of fever, in which Opium is admiſſible; let us next enquire what circumſtances demand its exhibition, and in what manner it is to be preſcribed.

After the inflammatory ſymptoms of fever have either ſpontaneouſly diſappeared, from the natural progreſs of the diſeaſe, or have been removed by blood-letting, if neceſſary, by antimonials, fomentations, the antiphlogiſtic regimen, and other means uſually employed for that purpoſe; the ſymptoms of debility, under which I would rank thoſe generally aſcribed to putreſcence, demand our principal attention, as the danger of the diſeaſe is now proportioned to their vehemence. To ſupport the neceſſary vigour of the ſyſtem while they prevail, a variety of ſtimulants have been recommended; but, by very general conſent, moſt of them ſeem, at preſent, juſtly ſuperſeded by wine, which, as a ſufficiently powerful and diffuſible ſti-

mulant, and at the ſame time durable, nutritious, and calculated to ſupport, as well as excite the vigour of the ſyſtem, ſhould be preferred to Opium as a *general* remedy in fevers, and indeed to every other ſtimulant we are acquainted with; joined to the Peruvian bark, where ſymptoms ſuppoſed to ariſe from approaching or preſent putreſcence indicate its uſe. It muſt, indeed, be acknowledged, that, in ſeveral caſes of typhus, Opium, when joined with other ſtimulants, as the volatile aromatic ſpirits, æther, &c. and given in frequently repeated doſes, has alone ſupported patients under the preſſure of conſiderable debility, and brought the diſorder to a favourable iſſue. It is with an intention of proving this circumſtance, that Dr. Wall's Caſes * have been given to the public, and ſome of them ſeem ſufficiently in point. I have myſelf, likewiſe, ſeen a ſimilar plan, in ſeveral inſtances, ſucceſsfully purſued; though I certainly ſhould

* Clinical Obſervations on the Uſe of Opium in Low Fevers, &c. by M. Wall, M. D.

never

never place my entire dependence on them, where wine could be procured.

Although wine, however, be preferable to Opium, as a *general* remedy in theſe complaints; yet, during their courſe, particular ſymptoms occur very univerſally, which, as experience ſufficiently teſtifies, abſolutely require the exhibition of Opium, or are at leaſt more ſpeedily and effectually relieved by it, than any other medicine we are acquainted with. Theſe we ſhall next particularly conſider; and, firſt, of

Delirium.—Although this very general ſymptom of fever may, perhaps, in ſome inſtances, ariſe from an inflammatory ſtate of the brain, or its membranes, and therefore require general or topical blood-letting, the application of bliſters and other remedies adapted to inflammation, and in a few others from impurities in the primæ viæ, when gentle emetics will afford relief; yet, in by far the greater number of caſes which occur, it will be found connected with general and urgent debility, and to require a very different mode of treatment. The de-

pendence of febrile delirium upon debility, is ſufficiently obvious from conſidering, that it is generally accompanied by a very frequent and weak pulſe; that it uſually occurs in diſeaſes attended with the greateſt proſtration of ſtrength, as the plague, confluent ſmall pox, cynanche maligna, laſt ſtage of gangrene, &c. and is, in ſuch caſes, beſt removed by ſtimulant medicines; and that it is in fevers frequently induced or aggravated by bleeding *. Its connection with debility is indeed ſo great that, if the latter ſucceed the inflammatory ſtage of fever to any conſiderable degree, it will be found among the firſt ſymptoms that indicate its preſence, as I have obſerved a few pages back; and it is accordingly alleviated by wine, and other medicines of a ſimilar nature: yet, although ſuch be well adapted to the cauſes of this ſymptom, in many inſtances they are found inadequate to its removal, which frequently requires the more powerful aſſiſtance of Opium. Sydenham, in

* Pringle, Diſeaſes of the Army, p. iii. ch. 6.

treating of the delirium which occurred in the epidemic fever that prevailed from 1661 to 1664, and which Cullen makes a ſpecies of ſynochus, obſerves, that he employed blood-letting, clyſters, and other cooling remedies, till the diſeaſe had run to a certain length, and that then he gave an opiate in a large doſe, and always with good effect; but that ſuch ſhould never be exhibited before the twelfth day. Whether the delirium occurring in the fevers he treated was really inflammatory, and required the evacuations recommended, I ſhall not pretend to determine; but can hardly imagine that fevers have at any time been ſo univerſally regular in their courſe, as to admit any general rule with reſpect to the preciſe day of exhibiting a powerful medicine. He was, however, ſo fully ſatisfied of the efficacy of the opiate when adminiſtered, that he ſays, " Non admodum difficile erit eum & a morbo & a ſymptomate (delirio ſc.) una eademque opera liberare, id quod fiet, narcoticum aliquod medica-

mentum in largiori paulo doſi exhibendo *."

Beſides the authority of Sydenham, the powers of Opium in allaying the delirium of the fevers of the preſent day immediately on its commencement, nay, in preventing its appearance where expected, are ſufficiently confirmed by that of ſeveral modern practitioners †; ſo that the cautions he lays before us ſhould not deter any practitioner from employing it, where required, at any period of the diſeaſe after the inflammatory ſymptoms are removed. Preventing the coming on of delirium as long as poſſible, is a matter of no ſmall importance; we ſhould therefore ſtudiouſly watch the appearance of ſuch ſymptoms as indicate its approach, and they are in general the following: The patient grows more

* Sydenhami Opera, ſect. i. cap. 4. § 43.

† See Gilchriſt. Med. Eſſays, vol. iv.——Cullen, Mat. Med. vol. ii. p. 234.——Campbell on Typhus, p. 80.——Blane, Diſeaſes of Seamen, part iii.——Wall's Clinical Obſervations.

reſtleſs and uneaſy than uſual; the pain in the head is increaſed, and attended with ſome confuſion; in ſome caſes a tinnitus aurium is complained of; ſhort ſlumbers, diſtributed by uneaſy and diſagreeable ſenſations and ideas, ſucceed long intervals of watching; and, in waking out of them, a degree of incoherence in the mind is obſervable. To theſe ſymptoms delirium very ſoon ſucceeds, if no precaution be taken to prevent it; but if opiates be exhibited, its appearance will not only be further protracted, but the ſymptoms themſelves conſiderably alleviated. The liquid laudanum may be given in any agreeable cordial julep, and repeated, as occaſion may require, till the patient falls into a ſound ſleep, which generally happens after one or two doſes. A ſimilar plan is to be purſued after the delirium has actually commenced; and it will be found always to moderate its violence, and ſo generally to remove it entirely, that the epithet of "ſovereign remedy" in thoſe caſes, ſeems to be applied to it with the greateſt propriety by Dr. Cullen. The

recurrence

recurrence of delirium, after being removed, will always require a ſimilar repetition of the opiate, and generally in an increaſed doſe.

Want of *ſleep*, which frequently precedes or accompanies delirium, is in like manner prevented or removed by the exhibition of opiates. Where it appears, however, in the inflammatory ſtage of the complaint, it is, perhaps, more effectually removed by fomentations than any other remedy.

Diarrhœa. This, as a ſymptom of increaſing debility, and by its effects ſtill further augmenting the cauſe which produced it, I would, in every inſtance after the ſtage of debility has ſupervened, reſtrain as ſoon as poſſible: the cautions with reſpect to checking it, which appear in ſome authors, appearing more the conſequences of hypothetical reaſoning, with reſpect to morbific matter, and its critical evacuation, than of fair experience; for this purpoſe no medicine ſeems equally efficacious as Opium, although it will be much aſſiſted by the addition of aſtringents. The opiates ſhould be

perſiſted

perſiſted in till productive of the deſired effect, and have ſometimes ſucceeded beſt when given by way of clyſter. Although, however, diarrhæa ſhould be checked, if poſſible, on its firſt appearance, yet it may here be remarked, that the natural evacuation from the inteſtinal canal ſhould not be prevented; but, on the contrary, if checked by opiates, or ceaſing ſpontaneouſly, ſhould be promoted by a mild clyſter injected daily, provided the patient's ſtrength be not much depreſſed. If, however, the debility be very urgent, we ſhould avoid every evacuation this way, the ſlighteſt being apt to aggravate it; and ſuch evacuation will be the leſs neceſſary, if the quantity of ingeſta has been trifling, the previous diſcharges regular or conſiderable, and the abdomen ſoft and free from tenſion. Sydenham ſays, when once the patient eſcaped the violence of the firſt ſymptoms, and the diſorder was on the decline, the more coſtive he was kept, the more ſecure he judged him *. And

* Sect. i. cap. 4. § 24. See alſo the ſame precaution repeated, ibid. § 39. and ſect. v. cap. 5. § 7.

Dr. Campbell confiders a coftive habit in fuch cafes as a favourable occurrence, having frequently feen patients three or four days without a ftool, and no bad confequences enfue *.

Tremor and *fubfultus tendinum*. Thefe are different degrees of the fame affection; the tremor which is generally firft perceived, gradually arifing to the degree termed fubfultus: both are dangerous fymptoms, indicating the prefence of urgent debility, and frequently terminating in death. If, however, a proper plan of treatment be purfued, the patient will frequently efcape after their appearance; and fuch entirely confifts in employing ftimulants to greater extent, and more diligently than before. Mufk has been recommended; but its high price, and confequent frequent adulteration, muft prevent its coming into general ufe. Camphor has been, in fome inftances, found ferviceable; but Opium is the principal remedy to be relied on. The opinion of Dr. Gre-

* On Typhus, p. 70. See alfo Heyfham on the Jail Fever, p. 54.

gory

gory is, on this point, decifive: "The remedies," fays he, "beft adapted to the removal of tremor and fubfultus, are wine pretty freely, and large dofes of Opium; which fhould be given without fcruple, and repeated as foon as the effect of the preceding dofe is beginning to be worn out. I have frequently feen the beft effects from them, in cafes where I did not expect the patient would live a few hours *."

The method here recommended of exhibiting Opium, fhould, in fuch cafes, always be purfued; for, by joining it with a more durable ftimulant, and frequently repeating the dofe, the excitement of the fyftem will be beft increafed and fupported, and the urgent debility and irritation, on which the fymptoms principally depend, alleviated or removed.

In further confirmation of the utility of the whole of the practice above recommended, I fhall ftate the leading circumftances of a cafe which I had myfelf an

* Lectures. See alfo Cullen, Materia Medica, vol. ii. p. 234.

opportunity

opportunity of treating, and which, among many others of a ſimilar nature which have occurred to me, places, I think, its efficacy in a very clear point of view.

C. G. a married woman, aged about thirty, and in the fourth month of her pregnancy, had been expoſed to the contagion of fever, and was very ſoon after ſeized with its uſual ſymptoms. I was called to her aſſiſtance the ſeventh day of her illneſs, previous to which little or nothing had been done, except injecting a clyſter now and then, as her body had been bound. I could obtain no ſatisfactory account of the previous progreſs of the diſeaſe, except that ſhe had been in general reſtleſs; but found her in the following ſituation: Her pulſes beat 120 in a minute, and were very weak; her tongue foul and dry; thirſt urgent; ſkin hot; eyes languid; and ſhe lay on her back in a ſtate of apparent ſtupidity. When any queſtions were put to her, ſhe was made anſwer them with difficulty, not wiſhing to be diſturbed; and her anſwers, when extorted, were frequently incoherent.

Twenty drops of Thebaic tincture were immediately given in about an ounce of Port wine; and a similar draught was prepared, with fifteen drops, to be taken in case she should not fall asleep in a few hours. A mild clyster was injected, which soon operated; she was ordered a slight solution of crystals of tartar in water, sweetened, as drink; and a free circulation of air was obtained, by throwing open a window at some distance from her bed. The next day, being the eighth of her disorder, I called on her early, and found that the first draught had soon thrown her into a sound sleep, which continued about six hours; out of which she awoke, as she said, somewhat refreshed, but some time after fell into a state of low muttering delirium, which still continued. The second opiate had not been given—her pulse was still 120; and there was little alteration in the other symptoms. She was ordered to take a table-spoonful of Port wine, mixed with an equal quantity of water, every second hour. A draught, with thirty drops of Thebaic tincture, was given her,

her, and by theſe means the delirium was ſoon removed; though ſhe did not fall into a ſound ſleep till about eight in the evening, when, as I did not wiſh to diſturb her, no opiate was given. Having ſlept till near midnight, ſhe awoke very reſtleſs and uneaſy; and, in the courſe of an hour after, miſcarried, with very little pain. I was ſent for, and finding the hæmorrhage ſtill continue, ordered cloths wet with cold water and vinegar to be applied to the pudendum, which ſoon reſtrained it:—her pulſe was 130. The wine was ordered to be omitted for ſome time, and an opiate draught, with thirty drops of Thebaic tincture was given her; in conſequence of which ſhe ſoon grew eaſy and fell aſleep, when I left her: ſhe grew delirious by morning. At ten I again viſited her, and found the pulſe 135, and very weak, the delirium ſtill continuing; tongue dry, foul, and ſcaly; face pale; ſkin, particularly of the extremities, rather cool; eyes languid; and a tremulous motion of her hands was perceptible, which ſoon increaſed to a beginning ſubſultus tendinum.

tendinum. Finding that, if ſome efficacious method was not purſued, the worſt conſequences were ſoon to be expected, I inſtantly gave her a tea-cup full of port wine warmed, containing forty drops of Tinct. thebaica, and twenty drops of the Spiritus volatilis aromaticus. The wine was ordered to be given in doſes of two ounces every ſecond hour; and a julep was preſcribed, of which one ounce was to be given, if neceſſary, every fourth hour, which contained fifteen drops of Theb. tincture. By two, a manifeſt change for the better was obſervable, when ſhe took a doſe of the julep; the hæmorrhage had not returned, the ſubſultus had diſappeared, and but a ſlight degree of tremor remained. The pulſe was 125, and much ſtronger; the ſkin all over the body warm, and gently moiſt; the delirium very much abated, and the patient compoſed; but the thirſt ſtill remained urgent: ſhe had ſome ſleep in the day, during which ſhe took no more of the opiate till ten in the evening, when two ounces of the julep were given her; theſe procured her a good

night's reſt. Next day, the tenth, her pulſe was but 115 and pretty ſtrong, ſkin gently hot and moiſt, thirſt leſs urgent, and tongue growing moiſt at the edges; delirium and tremor entirely removed, and countenance more natural; and, in the courſe of two days more, ſhe was reſtored to a ſtate of convaleſcence, by gradually diminiſhing the opiates, and perſiſting in giving moderate quantities of wine.

Intermittent Fevers.

Although the utility of opiates in intermittent fever has been made manifeſt principally by a few moderns, yet we find many of the more ancient phyſicians recommending them in ſuch complaints, either alone, or in conjunction with other remedies of a ſtimulant nature. Galen aſſerts, from his own experience, that the theriaca has frequently proved ſerviceable in them, as will be ſeen in ſeveral parts of his works, but more eſpecially where he treats in particular of the virtues of that medicine *. Alexander Tral-

* De Theriaca ad Piſonem, c. 15.

lianus likewiſe recommends opiates, joined to different ſtimulants, in the cure of quartans *. Wedelius †, Horſtius ‡, and other chemical writers, alſo inform us, that they have frequently given them with ſucceſs two or three hours before the expected approach of the paroxyſm; and Etmuller ‖ aſcribes conſiderable powers to them in the removal of every ſpecies of theſe complaints. But the good effects of Opium in intermittents, were not, indeed, fully confirmed till within theſe few years, during which the experiments of Monſ. Beryat, the ingenious Dr. Lind, Monſ. Duchanoy, and others, have been made public; which fully demonſtrate its utility, and which have been frequently repeated by other phyſicians, and with ſimi-

* Lib. xii. c. 8.

† Opiologia, lib. ii. ſect. 2. cap. 1.

‡ Problemat. Therapeut. decad i. quæſt. 7.

‖ Opium intermittentium paroxyſmos ſenſibiliter repellit, rigores potenter minuendo, & cætera ſymptomata mitigando. Diſſert. de Vi Opii Diaphoretica, cap. i. p. 7.——Friend alſo aſcribes a ſimilar power to Opiates, Emmenolog. cap. xiv. p. 167.

lar good effect. Beryat * recommends the exhibition of an opiate an hour before the expected commencement of the paroxyſm, eſpecially if the cold ſtage be vehement, and attended with conſiderable tremor and rigour; obſerving at the ſame time, that if given ſooner it will frequently fail; if during the hot ſtage, that it may prove dangerous. His fears, however, on this head, are groundleſs; for Dr. Lind has found, that opiates given half an hour after the formation of the hot ſtage, both rendered the remaining part of the paroxyſm milder, produced a more perfect intermiſſion, and rendered a leſs quantity of bark neceſſary to the completion of the cure †. Not long ſince, Monſ. Duchanoy publiſhed a memoir ‡, in which he has ſhewn, that opiates, given two hours

* Memoires de Mathematique & de Phyſique, &c. tom. ii. p. 24.

† Eſſay on the Diſeaſes of Europeans in Hot Climates, Appendix, p. 342. See this further confirmed by Skeete on the Bark, p. 180. and Dr. Odier of Geneva, Med. Comment. vol. vi. p. 352.

‡ Memoire ſur l'Uſage de Narcotiques dans les Fievres Intermittentes.

before

before the commencement of the paroxyſm, greatly expedite the cure; and we have further confirmation of their utility, when exhibited either before or after the commencement of the paroxyſm, in a publication lately given to the world by Dr. Murray of Gottingen *.

Although theſe authors, however, have ſufficiently proved the good effects of opiates given at any period of intermittent paroxyſms, they have not determined at what time they are beſt adminiſtered; and although Dr. Lind has ſhewn, that they are ſingularly uſeful when taken during the hot ſtage, yet as they cannot, when deferred to that period, entirely prevent the paroxyſm, an effect they are frequently productive of when given before its expected approach; and as, even if the paroxyſm ſucceeds their exhibition, they appear equally efficacious in rendering it mild and ſhort, as when adminiſtered during the hot ſtage, it would ſeem more adviſeable to exhibit

* Apparatus Medicaminum, vol. ii.

them before than after the paroxyſm has commenced. I look upon it, however, as impoſſible to lay down any certain rule, with reſpect to the time that ſhould intervene between the exhibition of the opiate and the acceſſion of the paroxyſm; for although we may, perhaps, in general, determine pretty exactly at what time the paroxyſm may be expected, yet this is by no means univerſally the caſe; the hour of acceſſion being, in many inſtances, very irregular, more eſpecially after the uſual progreſs of the diſeaſe has been interrupted by medical aſſiſtance: nay, were this even in our power, I ſhould conſider it improper to exhibit the opiate at ſo great a diſtance of time as two hours, or even one hour before the cold ſtage; for, as Opium exerts its ſtimulant properties very quickly after being taken, as various conſiderations lead us to conclude, that it is on ſuch its good effects in theſe caſes depend; and as, though conſiderable, they are ſpeedily exhauſted, the nearer its exhibition can be brought to the commencement of the paroxyſm, the better;

better; and, notwithſtanding we cannot determine the hour at which the paroxyſm will commence, the feelings of the patient will, in general, ſufficiently indicate its approach. It is moſt commonly preceded by a ſenſe of languor and debility, ſluggiſhneſs and uneaſineſs in motion, yawning, ſtretching, and ſometimes by a ſenſe of creeping on the ſkin. Theſe ſenſations, as the patient himſelf ſoon learns by experience, immediately precede the cold fit; and, on the firſt approach of them, the opiate ſhould be taken. From thirty to ſixty drops of the Tinct. Theb. will, in general, be the proper doſe for an adult, but may be varied as circumſtances require; and, given in this manner, I have frequently ſeen them prevent the formation of the paroxyſm entirely, and always, when it did take place, render its progreſs milder and ſhorter. Let it not, however, be imagined, that opiates ſuperſede the neceſſity of employing that only effectual remedy the Peruvian bark. It is ſolely meant to recommend them as powerful aſſiſtants, which expedite and facilitate the cure in

common cafes; and which, when the dif-eafe proves obftinate, and long refifts the powers of that generally efficacious medi-cine, render it, by their ftimulating pro-perties, at length fuccefsful *.

PHLEGMASIAE.

Under this title Dr. Cullen has ranked the diforders ufually termed *inflammations*; in all of which, as has been already ob-ferved, the greater number of medical writers have looked upon Opium as very prejudi-cial,

* On confulting a paper by the learned Sir George Baker, refpecting fome very obftinate intermittents which prevailed a few years fince in feveral parts of England, and in which the bark proved frequently in-effectual, I was furprifed to find he never employed Opium as an affiftant to it, nor even mentions its hav-ing been recommended by others. The only notice taken of it, is in a letter from Dr. Petrie, which he inferts, who mentions, that "*one* patient, juft at the "time when the fit was expected, took fixty drops of "Thebaic tincture, on which he fell into a profound "fleep, fweated profufely, and efcaped the fever, not "only then, but at two fucceffive periods." Lond. Medical Tranfactions, vol. iii. art. 12.

cial, at leaſt while the inflammation continues in any degree conſiderable; and this remark will be further confirmed by what we are to offer on its employment in particular genera of this claſs, to be hereafter treated of. With reſpect to inflammation in general, however, a few remarks ſeem neceſſary to be premiſed before deſcending to particular inſtances.

Inflammation may be, and has been, divided into two ſpecies, active and paſſive; the firſt ſeeming to ariſe from a partial or general increaſe of excitement and vaſcular action, the ſecond being connected with partial or general debility. Of the former, pneumonia, gaſtritis, acute rheumatiſm, and other ſimilar diſeaſes, are inſtances; of the latter we have examples in the inflammation of the tonſils attending the cynanche maligna, in ſome ſpecies of ophthalmia, in ſcrophulous inflammation, and in chronic rheumatiſm. As theſe two ſpecies of inflammation differ ſo materially in their natures, ſo the modes of alleviating or removing them are directly oppoſite: in

active

active inflammation our principal aim being to lower the excitement of the syſtem, and diminiſh vaſcular action by blood-letting, and other evacuations; by avoiding all ſtimulants, and ſtrictly adhering to the antiphlogiſtic regimen: while, in the paſſive, our intentions are to remove univerſal or partial debility and atony by ſtimulants and aſtringents, generally or topically applied. Although ſtimulants, however, are in general to be avoided in every ſpecies of active inflammation, yet this prohibition is not to be underſtood without exceptions; for as, during the courſe of ſome of theſe diſeaſes, it is found advantageous, beſide other evacuations, to increaſe that made by perſpiration or ſweat; and as, in order to promote the latter diſcharge, particular ſtimulants are generally requiſite, they may be, and are frequently exhibited under ſuch circumſtances to advantage. It is, however, to be noticed, that they are not given in ſuch caſes with a view to the ſtimulant effects they produce, but with an intention of beneſiting by the evacuation they are finally

productive

productive of. Stimulants are likewiſe admiſſible or neceſſary in the latter ſtages of active inflammation, after the preceding increaſe of excitement has been followed by an oppoſite ſtate, of which we have a ſtriking inſtance in rheumatiſm; and theſe, indeed, ſeem the two principal exceptions to the general rule, which directs us to avoid them in active inflammatory diſorders, the particular genera of which we ſhall next proceed to conſider.

Pneumonic Inflammation.

Under which I comprehend both the ſpecies of Pneumonia and Pleuritis, is a diſorder which demands particular attention. Organs the moſt neceſſary to life and health, are directly expoſed to its violence, and a ſlight increaſe of inflammation may immediately endanger life, or lay the foundation of incurable diſeaſes. The degree of inflammation, likewiſe, which attends it, is generally conſiderable, and, if not ſpeedily alleviated, may terminate fatally in a very ſhort ſpace of time; and hence phyſicians

have almoſt univerſally agreed in recommending blood-letting, as the moſt effectual means of reducing the inflammation, from the exceſs of violence in which, the worſt conſequences immediately reſult.

An almoſt equal coincidence of opinion is obſervable among medical writers with reſpect to the employment of opiates in this complaint; it being generally remarked, that in the early ſtages, while the inflammation continues violent, they are ſtudiouſly to be avoided, as aggravating the cauſes from which the greater number of the ſymptoms ariſe, and from the exceſs of which, danger is principally to be apprehended *; and, indeed, when the violence of the diſorder, the organs it affects, and the ſtrong ſtimulant powers which the medicine poſſeſſes, are taken into conſideration, the precautions with reſpect to its exhibition, which have

* See Van Swieten Comment. in Boerhaavii Aphoriſmos, 854, 889.———Cullen's Firſt Lines, ſect. 375.———Mat. Medica, vol. ii. pag. 237.———Young on Opium, ſect. 4.———Pringle's Diſeaſes of the Army, p. 3. ch. 2.———Trallian. lib. vi. cap. 1.

been extended to every inflammatory complaint, ſeem particularly applicable to the diſeaſe in queſtion, and ſhould render us highly cautious in what ſtage of the diſorder we recommend its exhibition. Theſe remarks, as tending ſtrongly to ſupport the opinions advanced in the preceding chapter, may, perhaps, appear to ſome rather the reſult of theoretical ſpeculation than of practical knowledge; but when it is conſidered that the authors quoted were unacquainted with, or averſe to the opinion in queſtion, and that its bad effects in the circumſtances alluded to, have been actually experienced by many, ſuch ſuſpicion muſt naturally be deſerted. There have not, however, been wanting ſome to preſcribe Opium in the earlier ſtages of pneumonic inflammation. De Haen recommends a mixture compoſed of linſeed oil and opium *. Dr. Huxham ſeems likewiſe inclined to favour their uſe in the ſame complaints †; and a Dr. Hamilton

* Ratio Medendi Continuat. tom. i. cap. 2. verſus finem.

† On Pleuriſies, chap. 4.

milton of Ipſwich aſſerts, that opiates joined with mercurials and antimonials are highly uſeful in all inflammatory diſorders *. But let it be remarked, that theſe writers generally adviſe caution in their exhibition, and always recommend previous evacuation by blood-letting; and after the inflammation has been, by this and other means, removed, it is univerſally agreed that opiates are uſeful in alleviating many diſagreeable ſymptoms. "It is true, indeed," ſays Huxham, ſpeaking of their exhibition in pleuriſy, "theſe demand ſome degree of caution and prudence in the uſe of them; therefore ever remember, before you enter upon them, the lancet is not to be ſparingly uſed:" and De Haen only recommends them, "poſt ſufficientes ſaltem copioſas evacuationes." We ſhould not, however, have recourſe to them immediately after veneſection, as a frequent repetition of that operation is often requiſite, the inflammation ſtill continuing conſiderable; and while that is the

* Medical Commentaries, vol. x.

caſe,

caſe, I muſt ſtill conſider both opiates and mercurials improper; being conſiderable ſtimulants, they can only prove uſeful by the evacuation they may finally produce; but, as this is preceded by a ſtimulant operation, which may be productive of the worſt effects before that evacuation follows, or may prove ſo conſiderable as not to be finally compenſated, they ſhould ſurely be deferred till every danger from inflammation is over. Nor do the caſes produced by the authors juſt quoted, by any means prove that Opium is adapted to the inflammatory ſtage of pneumonia. In all of them the principal remedy, blood-letting, was previouſly employed; and the imminent danger being thus removed, it by no means follows, that becauſe they perceived no bad conſequences none really took place; or, becauſe the patient recovered, it aſſiſted, thus exhibited, his cure. Many have been the errors occaſioned by attributing ſalutary effects to every article employed during ſuch diſeaſes as the patient recovered from. Sydenham aſcribed the life of thoſe who, under his hands,

hands, got through the plague, to the copious venefection he employed; and Van Helmont attributes the recovery of fuch of his patients as fortunately efcaped the dangers of pneumonic inflammation, to a few ounces of dried goat's blood.

But although, while the inflammation continues in any degree confiderable, opiates are improper in pneumonia, yet in the latter ftages of the complaint they are particularly ufeful, efpecially when joined with mucilaginous or oily demulcents. The circumftances indicating their employment are fo concifely noticed by the accurate Dr. Cullen, that I fhall tranfcribe his fentiments, which ftrongly corroborate the opinions juft advanced. "To me it appears," fays he, "that in the beginning of the difeafe, and before bleeding and bliftering have produced fome remiffion of the pain, and of the difficulty of breathing, opiates have a very bad effect, by their increafing the difficulty of breathing and other inflammatory fymptoms. But in a more advanced ftate of the difeafe, when the difficulty of breathing has abated, and when the urgent

urgent ſymptom is a cough, proving the chief cauſe of the continuance of pain and want of ſleep, opiates may be employed with great advantage and ſafety. The interruption of expectoration, which they ſeem to occaſion, is for a ſhort time only, and they ſeem often to promote it, as they occaſion a ſtagnation of what was by frequent coughing diſſipated inſenſibly, and therefore give the appearance of what phyſicians have called concocted matter *. The general principles above laid down will alſo be ſufficient to direct our practice with reſpect to Opium in catarrhal affections; in the commencement of ſuch complaints it is always prejudicial. I have in my own perſon more than once experienced it. After the inflammatory affection has been allayed, if the cough continue troubleſome, it may prove ſerviceable by allaying the irritation, which, in the latter ſtage, ſeems one principal cauſe of the continuance of the complaint.

* Firſt Lines, ſect. 375.

Rheumatism.

This diſorder has been very properly divided into the acute and chronic; the former ariſing from an inflammatory ſtate of the parts affected; the latter, moſt probably, from an oppoſite, or that of atony and debility. The cure of the acute, as an inflammatory affection, is generally begun by blood-letting; but phyſicians have differed with reſpect to the extent to which this remedy ſhould be employed; general rules, however, can never, in ſuch inſtances, be laid down with propriety; ſome conſtitutions and degrees of the diſorder requiring much more copious and more frequently repeated evacuations than others. It is not, however, adviſeable to puſh blood-letting too far, as a very profuſe evacuation this way will either induce the chronic rheumatiſm, or be productive, perhaps, of even more diſagreeable effects.

The plan of cure at preſent moſt generally adopted, and moſt efficacious, is as follows: After the violence of inflammation has been

abated

abated by blood-letting general and topical, and the other uſual means, if the pains ſhould ſtill prove troubleſome, the patient is to be thrown into an univerſal ſweat, which is to be continued for 12, 24, or 36 hours, and repeated if the obſtinacy of the complaint require it. The inflammatory diatheſis of the ſyſtem ſhould, however, be well ſubſided before we have recourſe to this expedient. I have ſeen ſudorifics, prematurely employed, aggravate the complaint, and render more bleeding neceſſary than would, perhaps, have otherwiſe been required; after which operation, however, they were again had recourſe to with good ſucceſs. If ſweating could be invariably excited and ſteadily ſupported, without employing medicines of the ſtimulant kind, it is reaſonable to expect that it would prove both ſafer and more effectual in inflammatory complaints, than when produced by their operation. Stimulants, however, are generally required when we wiſh to excite or continue this evacuation; but, as rheumatiſm affects not any of the parts

immediately neceſſary to life, as in the diſeaſe laſt treated of, and as the ſtimulant operation of the ſudorifics we employ, is ſoon ſucceeded by the evacuation deſired, the exhibition of them is attended with no danger, and their ſtimulant operation is fully compenſated by the ultimate effect they produce.

Should this effect, however, not ſucceed, we can only expect an aggravation of the complaint; and we ſhould therefore be led to employ ſuch ſtimulants as by experience have been found moſt invariable in their operation as ſudorifics. Of all hitherto recommended, the compoſition termed Dover's Powder, or Pulvis Ipecac. comp. (L), of which Opium and Ipecacuana are the principal ingredients, is the moſt effectual; and very rarely fails, if properly managed, to produce the deſired effect. This compoſition was firſt invented and recommended as a powerful remedy in acute rheumatiſm by Dr. Dover; and daily experience ſufficiently juſtifies his opinion. The moſt proper mode of exhibiting it, is ſo very

fully

fully and juftly laid down by Dr. Cullen, in his Materia Medica, that I fhall only refer the reader to the 259th page of the fecond volume of that work. Adminiftered in the manner there recommended, after the violence of the inflammatory diathefis had been fufficiently allayed by blood-letting, I have always found it, in acute rheumatifm, a very effectual remedy. Although Opium, thus prefcribed, is, however, a medicine of real utility in the complaint under confideration; yet, where not exhibited in fuch a manner as to procure an evacuation by the fkin, but merely with an intention of allaying pain, it will be found prejudicial. Of this I cannot fay I have myfelf obferved many decifive examples, as I have never given it with fuch intention, and have only feen two inftances of a contrary practice in others, in both of which it evidently aggravated what it was intended to relieve. One proof, however, of the juftice of this practical caution I have frequently obferved, which is, that after the exhibition

of Dover's powder, or Opium in any other form, in acute rheumatiſm, the pains are generally increaſed before the ſweat breaks out; the flowing of which, however, immediately gives relief. But that opiates are highly prejudicial in the acute rheumatiſm, when not employed as ſudorifics, is evident from the joint teſtimonies of the moſt celebrated practitioners. Sydenham's expreſſions are on this point very ſtriking: "Quin et anodyna," ſays he, "ſive paregorica dicta remedia effuſiorem ſanguinis miſſionem poſtulant: quamobrem ut ut ſæviat dolor, per omnem hujus morbi decurſum religioſe mihi ab iis temperandum exiſtimo, modo mihi animo ſit curationem a ſanguinis miſſione ſolum petere; cum illorum uſu figatur morbus, neque tam facile vænesectioni cedat; quæ ideo, ob hæc ipſa medicamenta officioſe nimis exhibita, ſæpius quam erat opus celebranda eſt. Quid quod & in morbi ſtatu neque eam quam titulo pollicentur ἀναλγησιαν valeant præſtare *. Cullen,

* Opera Medica, ſect. v. cap. 5. p. 274.

len *, Van Swieten †, Pringle ‡, Storck ‖, Macbride §, and many other authors, concur in the ſame ſentiments, and ſufficiently eſtabliſh the practical caution above laid down.

Although, while the diſeaſe, however, is really of the acute inflammatory kind, our practice, with reſpect to Opium, ſhould be regulated by the above remarks, I have obſerved a ſpecies of the diſorder, which Dr. Skeete notices ¶, and which he terms the *acute rheumatiſm with irritability* ; in a few caſes of which I have obſerved ſuch good effects from Opium, that I think it may be ſafely recommended to the trial of other practitioners. This ſpecies of the diſorder often ſucceeds the really acute after repeated evacuations ; ſometimes attacks perſons, women eſpecially of an irritable and deli-

* Firſt Lines, § 468. Mat. Med. vol. ii. p. 239.

† Commentaria, &c. § 1493, 1495.

‡ Diſeaſes of the Army, part iii. chap. 2.

‖ Annus Medicus Secund. p. 120.

§ Methodical Introduction, &c. book iv. ch. 2.

¶ On Bark, p. 237.

cate habit, without any preceding very inflammatory ſymptoms; and is, in ſhort, that modification of the diſeaſe which muſt have ſtruck every attentive practitioner, in which local pain, ſwelling, and redneſs are ſo complicated with general debility, a quick pulſe, and other ſimilar ſymptoms, that he is ſomewhat uncertain which ſpecies of the complaint to refer it to, or what plan to purſue. In a few caſes of this kind, I have ſeen bark, as recommended by Skeete, of ſervice; and it appeared to have been materially aſſiſted by opiate draughts conjoined with antimonial wine, as recommended by Fothergill * in the ſciatica; twenty-five or thirty drops of the former to thirty of the latter. In a few other caſes I have given draughts of this nature alone twice a day, which in a few days effected a cure: they generally induced a heat and moiſture of the ſkin, but ſeldom a ſweat; the patients were kept warm, and afterwards gradually admitted to the free air, and had no return of the complaint.

* Medical Obſervations, vol. iv. art. 5.

Gout.

With reſpect to the employment of Opium in the regular ſpecies of this complaint, I cannot venture to offer any thing deciſive; all the older practical writers forbid its uſe; and, perhaps, while it continues to attack the extremities alone, it is ſafer to truſt it, as is commonly done, to patience and flannel. On the other hand, Dr. Brown and many of his followers aſſert, that the gout, in all its forms, ariſes from debility of the indirect kind, is eaſily removed by powerful ſtimulants; and, of conſequence, that Opium in large and frequently repeated doſes, is a remedy, with other ſtimulants, peculiarly adapted to its removal in every ſtage. The arguments in favour of theſe opinions, are deduced from the advanced age and debilitated conſtitutions of thoſe who are generally the ſubjects of the complaint; from paroxyſms being frequently brought on by cathartics, blood-letting, low diet, and other debilitating powers; from the neceſſity which is on all hands acknowledged,

acknowledged, of giving the moſt powerful and diffuſible ſtimulants when it attacks the ſtomach, inteſtines, or any of the vital organs; and from the alledged experience * of

* If the ſpecies which Dr. Cullen calls the *miſplaced gout*, Firſt Lines, § 523, 524, 536, really requires the large evacuations many authors recommend, it will militate ſtrongly againſt this argument. One caſe, which occurred in my own family the firſt year I entered on the ſtudy of medicine, appears ſo ſtrongly in proof of the propriety of the practice, and the inflammatory nature of this ſpecies of the gout, that I ſhall beg leave to ſtate it. A married lady, aged about forty-five, in whoſe family the gout had been very prevalent, was ſeized with a painful ſwelling and redneſs in both feet, but more violent in one than the other; it extended from the extremity of the toes upwards to the ankle joint; and the pain, which ſhe compared to that ariſing from a ſcald or burn, became at length ſo violent, that a phyſician was ſent for; but, being abſent, an apothecary was called in, who pronounced it a rheumatiſm, and recommended the external application of camphorated ſpirits of wine. This was accordingly uſed, and in the ſpace of a few hours completely removed the complaint; but, in a very ſhort time afterwards, ſhe was ſeized with a cough, dyſpnœa, pain in the ſide, and other ſimilar ſymptoms of an affection of the lungs, which ſoon increaſed to an alarming

of Dr. Brown, and ſome of his pupils. Having never ventured on ſuch practice myſelf, nor in any inſtance ſeen it purſued by others, I ſhall not attempt to offer any opinion with reſpect to its propriety: on extenſive experience alone ſhould ſuch an

ing degree, on which my intelligent friend, Dr. Leyne, was ſent for: he inſtantly had her blooded, and was obliged to repeat the operation twice in the courſe of the two ſucceeding days; at the end of which time a copious expectoration commenced, and the diſorder terminated favourably ſhortly after. Bliſters were applied to the legs with a view of renewing what had the appearance of a gouty affection, but to no purpoſe. In the courſe of ſome time, however, it ſpontaneouſly returned; and, being left to nature, it went off after ſome weeks continuance. The lady is ſtill alive, and ſubject to returns of the ſame affection, which recur at uncertain periods, and are generally preceded, as the regular gout is, by affections of the ſtomach and alimentary canal; ſometimes by vertigo and dimneſs of ſight. No remedies are ever employed; and the ſymptoms diſappear after ſome weeks continuance. On the other hand, I have heard of two inſtances, but to which I was not myſelf a witneſs, of pain in the ſide and dyſpnœa occurring in gouty habits, for which the patients were bled, but with the worſt conſequences; both expired in a very ſhort time.

opinion be founded: till this occurs (which, from the popular ideas with reſpect to the nature of the complaint, and the ſituation in life of thoſe generally afflicted with it, will not eaſily occur), it is better to acknowledge uncertainty and doubt, than precipitately and dogmatically to offer any deciſion.

In that ſpecies of the gout called the Irregular, Atonic, or Retrocedent, or, as it is popularly termed, the gout in the ſtomach, from the violent affections of that organ, no doubt remains with reſpect to the propriety of exhibiting Opium: like other powerful ſtimulants, it affords the greateſt relief, and ſhould never be neglected. It is ſometimes neceſſary to give it in very large doſes; and, like every other diffuſible and eaſily exhauſtible ſtimulus, it ſhould be frequently repeated *.

* Cullen, Firſt Lines, ſect. 580, 581. Materia Medica, vol. ii. p. 262.———Sydenham. Tractat. de Podagra, p. 474.———V. Swieten. Comment. in aph. 1281.———Coſte, Traité Pratique ſur le Goutte, p. 72.

SMALL-

Small-Pox.

In the mild and diſtinct ſpecies of this complaint, whether ariſing from inoculation or natural infection, Opium is hardly ever requiſite; nay, from the inflammatory nature of the diſeaſe, and the utility of purgatives and the antiphlogiſtic regimen, it muſt prove prejudicial, and is accordingly very univerſally omitted. In the confluent ſmall-pox, on the contrary, no medicine is more frequently employed, or productive of more beneficial effects. This, however, is to be underſtood of the ſuppurative and ſubſequent ſtages chiefly; for, although the fever attendant on the confluent ſmall-pox be very univerſally of the typhus kind, yet, like the genuine typhus, it commences with inflammatory ſymptoms, which generally continue till the fourth or fifth day; before which period, opiates are generally improper. An exception to this rule is, however, to be made where frequent and violent convulſions precede the eruption; for although a few ſlight fits at this period are conſidered

confidered as favourable fymptoms *, where they are confiderable, and recur at fhort intervals, they are in themfelves frightful and dangerous, and generally portend a confluent and typhous fmall-pox. They fhould, therefore, if poffible, be immediately quelled; and for this purpofe, nothing is fo efficacious as a large dofe of an opiate †, which it is generally neceffary to adminifter to children by way of clyfter.

After the inflammatory fymptoms of the eruptive fever have difappeared, which in this fpecies generally happens on the fourth or fifth day, an opiate fhould be given generally twice, frequently three times, in the courfe of twenty-four hours. I need hardly mention, that it is to Sydenham we principally owe this improvement in the treatment of the fmall-pox; and fucceeding phyficians have very univerfally experienced the good effects of the practice. Opium

* Cullen, Firft Lines, fect. 620.——Sydenham. Opera, p. 132.

† Cullen, ut fupra.——Sydenham. Differt. Epiftolaris, p. 368.

will alſo be materially aſſiſted by a medicine, which though Sydenham knew, and exhibited freely in other caſes, he did not preſcribe in this complaint; I mean the Peruvian bark, for the uſe of which, in ſmall-pox, we are indebted to Morton and Mead, but principally to Monro ſenior: theſe, conjoined with wine, are indeed the principal remedies to be relied on; and their general employment, by modern practitioners, renders the diſorder much leſs violent and mortal than was formerly the caſe; and appears conſiderably to diminiſh the proportion of thoſe violent putrid caſes remarked by the more ancient writers, where blood iſſued from almoſt every outlet, and which, no doubt, were frequently to be attributed to the almoſt indiſcriminate blood-letting they recommend.

The ſecondary fever which ſucceeds the diſtinct ſmall-pox, a circumſtance which rarely happens, is generally of the inflammatory kind; and accordingly opiates are to be avoided, and the treatment conducted on the antiphlogiſtic plan. Where the ſe-

condary

condary fever follows the confluent ſmall-pox, however oppoſite the opinion may be to the authority of Sydenham and many others, bleeding, and every other debilitating power, appear to me abſolutely pernicious *, and acids, bark, wine, and opium, the only remedies to be relied on. This opinion is ſupported by the authority of Doctors Cullen and Gregory; and the ſucceſs I have obſerved attend it, confirms to my mind the propriety of the practice. In this ſtage of the ſmall-pox a diarrhœa very frequently occurs, with reſpect to the checking of which by opiates, practitioners have materially differed. Morton conſidered it, when preceded by ſpitting, as an infallible ſign of death, unleſs it could be checked †; and

* It is very remarkable, that the great Boerhaave, who in general pays every attention to Sydenham's practice, never mentions bleeding in his Aphoriſm on the Cure of the ſecondary Fever of the confluent Small-pox, though V. Swieten, in his Commentary, is an advocate in its favour; and that, in the ſucceeding aphoriſm, he laments the abſolute inutility of the practice at that time generally followed. See Aph. 1402, 1403; and V. Swieten's Commentary.

† Pyretalogia, cap. ix. p. 96.

recommends

recommends avoiding all evacuation this way. Sydenham does not appear to adviſe any large evacuation from the belly, but merely to keep it naturally open towards the end of the complaint *. Friend, on the other hand, has left an expreſs Treatiſe on the Utility of Purgatives in this complaint †. Mead ‡, Huxham ‖, De Haen §, Van Swieten ¶, and many others, look upon a natural diarrhœa as a ſalutary and critical effort of nature; and when it does not ſpontaneouſly come on, recommend the repeated exhibition of purgatives. I recollect hearing the late Dr. Cullen obſerve, that, in his younger days, the practice recommended by Friend and Mead, of purging in the ſecondary fever, was univerſally adopted; that he, among others, conſtantly purſued

* Opera, p. 550, 551. De Febre Putrida Variolis ſuperveniente.

† De Purgantibus in ſecunda Variolarum confluentium Febre.

‡ De Variolis & Morbillis.

‖ On Small-pox, p. 163.

§ Ratio Medendi Continuat. tom. i. cap. 12.

¶ Comment. in Boerhaav. aph. 1402.

it, but found it ſo inefficacious, that he had totally deſerted the practice, and latterly truſted principally, and with much more ſucceſs, to bark, wine, and acids. With reſpect to my own obſervations, and I have ſeen a good many caſes of the confluent ſpecies of the diſorder, although coſtiveneſs was always avoided, and a regular diſcharge from the inteſtinal canal kept up, I have conſtantly in adults checked a diarrhœa on its very firſt appearance by opiates, and truſted the cure of the complaint to theſe, bark, and wine. As an inſtance, I ſhall ſtate the leading circumſtances of a caſe which occurred to me, and the ſubject of which, according to the directions of the authors above quoted, required the evacuations both of bleeding and purging.

I. F. an unmarried girl aged 19, robuſt, plethoric, and healthy, took the infection of the ſmall-pox naturally, which proved very confluent. I did not ſee her till the twelfth day from the firſt appearance of the eruption. She had taken no medicines from the commencement of the complaint but

one

one bowl of wine-whey, and a few glaſſes of warm punch; which had been given after the eruption was completed. The puſtules, which were beginning to cruſt, were uniformly confluent all over the face, ſo that no interſtices could be obſerved; on the body they were leſs ſo; on the extremities, where theyhad not broke, they were flat, irregular, and confluent; but where interſtices appeared they had ſomewhat of an inflamed margin. Her pulſe was 120 and of good ſtrength, ſkin very hot, tongue parched, thirſt urgent, belly very looſe for twelve hours, and the diarrhœa increaſing; breathing rather laborious, had been delirious for about half an hour in the morning, was ſtill incoherent, and had been almoſt totally deprived of ſleep ſince the commencement of the complaint. This was in the evening. She inſtantly got 25 drops of Theb. tincture in a tea-cup full of port wine, which checked the diarrhœa, and ſhe ſlept pretty well. Next morning ſhe got 20 drops in the ſame vehicle, which entirely ſuppreſſed it; and ſhe began the

 bark,

bark, of which ſhe took a drachm four or five times a day in a cup of the ſame cordial; ſhe took the opiate regularly twice a day, and had plenty of ſmall beer allowed for drink. She continued in a ſtate of progreſſive amendment, neither delirium nor diarrhœa returned, and in ſeven days was in a ſtate of perfect convaleſcence. Many other very ſimilar inſtances have occurred to me.

MEASLES.

In this complaint the uſe of Opium is very limited, it is clearly and avowedly of a very inflammatory nature; and Sydenham, as Dr. Cullen juſtly obſerves, was probably more led by the analogy ſuppoſed to exiſt between it and ſmall-pox, than by the fair reſult of actual practice, to recommend it as he has done during its early ſtages. Convinced of the inflammatory nature of the complaint, and the ſtimulant qualities of the medicine, I have never given it unleſs in the extreme periods of the diſorder, and think the cough will in

the earlier ſtages, if very violent, be more effectually appeaſed by blood-letting, bliſters, and mild demulcents, than by Opium in any form. Another ſymptom occurs in meaſles which we ſhould never venture to check by opiates, and which very generally relieves the patient; I mean a diarrhœa. Of this a very ſtriking inſtance occurred to me lately. A lady aged above 50, after expoſure to the contagion of meaſles, was ſeized with the uſual ſymptoms of the complaint, but which continued to increaſe to a very alarming degree. The cough was violent, painful, and inceſſant, and accompanied by a conſiderable oppreſſion and difficulty of breathing. Theſe continued to increaſe till five o'clock in the evening of the day on the morning of which the eruption had appeared, at which hour ſhe was ſeized with a violent diarrhœa, which very ſhortly relieved the thorax, in ſo much that the pain, cough, and dyſpnœa were ſoon entirely removed, and never after returned in the ſlighteſt degree. She was purged above fifty times in the courſe of the night;

 I arrived

I arrived next day about twelve: the diarrhœa ſtill continued, but was very moderate and decreaſing; pulſe regular and of good ſtrength; eruption complete. I would not check the diarrhœa, as it had produced ſuch ſtrikingly good effects, and her ſtrength was not impaired; it ceaſed ſpontaneouſly before night, when, as ſhe complained of great uneaſineſs in her bowels, and all inflammatory ſymptoms had entirely ceaſed, I ventured to give her fifteen drops of Theb. tincture, which compoſed her; ſhe paſſed the night very well, and ſhortly after perfectly recovered.

HÆMORRHAGIES.

The diviſion of this claſs of diſeaſes into active and paſſive is well known, and by keeping it in view we ſhall be enabled to regulate the employment of Opium in ſuch ſpecies of them as require its uſe. In the firſt, or active, which are preceded and accompanied by a ſmart pyrexia, quick full pulſe, and other ſimilar ſymptoms, it is, like every other ſtimulant, to be refrained from.

In

In paſſive hæmorrhagies, on the contrary, whether of that nature originally, or ſucceeding thoſe of the active kind, it may be, and frequently is given to advantage. This is particularly the caſe in the Menorrhagia ſucceeding delivery, and I have found it equally advantageous in violent caſes of that complaint occurring in the unimpregnated ſtate. In *Hemophyſis* a vehement cough ſometimes renders it neceſſary; but the phlogiſtic diatheſis ſhould be well allayed before we venture on its uſe. In *Epiſtaxis* I have never ſeen it given, nor had occaſion to try it myſelf; but a ſkilful practitioner has informed me that, in a very violent caſe, which reſiſted every other remedy external and internal, and had very much reduced the patient, he found a large doſe of Opium immediately effectual, and had heard of ſimilar inſtances from others. In *Hemorrhois* I know not whether it has been employed internally; it has ſometimes been given externally, in the form of a liniment.

PHTHISIS.

In this complaint, Opium, like almoſt every other medicine, appears more neceſſary as a palliative, than advantageous as a radical and effectual remedy. In the commencement of the diſorder, when any increaſe of the phlogiſtic diatheſis of the ſyſtem may accelerate the ſuppuration of tubercles, it is to be cautiouſly, if at all employed; but in the more advanced ſtages of the complaint, where ſuppuration has taken place, and a troubleſome cough and diarrhœa occur, little doubt can be entertained of the propriety of its exhibition. By quieting the cough, allaying the diarrhœa, and inducing ſleep, it at leaſt retards the fatal termination of the complaint, or renders its approach more gentle, and leſs diſtreſſing. Some have ſuppoſed that it increaſes the ſweats which ſo univerſally occur; but this effect I have very generally found corrected by the addition of the vitriolic acid.

Dysentery.

The real uſe and proper mode of exhibiting Opium in this complaint, ſeem by no means perfectly underſtood; nor is there any diſorder in treating of which I feel ſo much at a loſs in offering my ſentiments on theſe points. This embarraſſment ariſes from two ſources: the contrariety of opinions entertained by many reſpectable writers as to its utility, and my not having yet met in practice a ſufficient number of well marked caſes of the complaint to enable me to fix or regulate my judgment. A very great number of authors, ancient and modern, practical and theoretical, condemn the general uſe of opiates in dyſentery; allow it only to be given when the pain and irritation of the inteſtines are very violent, and conſider the relief it then affords as merely temporary, and ſometimes productive of diſagreeable conſequences *.

Others,

* Trallian, lib. viii. cap. 8.——Cleghorn, Diſeaſes of Minorca, chap. 5.——Pringle, Diſeaſes of the

Others, however, are not wanting who recommend its more early and repeated uſe, and ſeem to truſt chiefly to its efficacy for a cure. Of theſe the principal is Sydenham, whoſe opinion of its utility in the cure of dyſentery was founded on very extenſive practice. He began the cure with a few gentle purges, giving at the ſame time an anodyne draught, morning and night, which generally removed the diſeaſe in a ſhort time; if, however, it proved obſtinate, he gave 25 drops of his Laudanum * every eighth hour, beſides uſing every day an injection, compoſed of half a pint of milk, and one ounce and a half of Theriaca An-

the Army, p. iii. ch. 5.——Blane, Diſeaſes of Seamen, part iii.——Baker de Dyſenteria.——Degner. Hiſtoria Medica de Dyſent. Bilioſ. &c.——Cullen, Firſt Lines, vol. iii. ch. 2.——Moſely, Treatiſe on Tropical Diſeaſes.

* This, it muſt be obſerved, was ſtronger in the proportion of one third, than the ſame quantity of our Thebaic tincture; as Sydenham uſed but 1lb. of menſtruum to two ounces of opium; we employ $1\frac{1}{2}$ lb. to the ſame quantity. His 25 drops were, therefore, a ſtronger doſe than 33 of our tincture.

dromachi. This plan he found very universally effectual; and so far from experiencing any bad consequences from the repetition of the opiate, he says, " Nec vel minimum quidem incommodi, a tam frequenti medicamenti narcotici repetitione mihi adhuc videre contiget (quantumlibet noxam inde sequuturam comminiscantur *inexperti)*; licet plures noverim, qui in morbo contumaciori, idem ad septimanas aliquot continuas quotidie usurpaverint *." And at the conclusion of the chapter he thus expresses himself: " Antequam finiam, hoc est notandum, quod, tametsi in his annis quibus dysenteriæ adeo epidemicæ grassarentur, evacuationes prius memoratæ prorsus necessariæ sunt, antequam ad usum laudani deventum fuisset: attamen, in constitutione quavis huic morbo minus faventi, istæ tutò omitti possint; ac curatio compendiosiori via, solo nempe usu laudani absolvi, eo quem descripsimus modo †." In his Schedula Monitoria, written above twelve

* Opera omnia, p 185. † P. 189.

years

years after, during which time it muſt be ſuppoſed he had ſufficient experience to aſcertain the real merits of this mode of practice, his expreſſions in its favour are ſtill more ſtriking. After condemning perſeverance in the uſe of repeated purgatives, he ſays, "Periculoſiſſimum mihi conſtat, morbum aggredi protractiore illa methodo; —cum *experientia* didicerim, dyſenteriam & certiſſime & celerrime curari, ſi, miſſis ambagibus, fluxus laudano confeſtim ſiſtatur *." He then repeats directions ſimilar to thoſe already laid down, recommending, beſides a moderate uſe of wine, fleſh broths, eggs, and other articles of diet of eaſy digeſtion. In warm climates, where the dyſentery is both more violent and frequent than in theſe kingdoms, Lind ſays that an earlier uſe of Opium than we are accuſtomed to is neceſſary †. Wedelius ſays, that Opium is the only remedy for dyſenteries ‡; and Bontius aſſerts, that in

* P. 531.

† Diſeaſes of Europeans, &c.

‡ Opiologia, lib. ii. ſect. 2. cap. 5.

the

the Eaſt Indies he found nothing ſo ſpeedily or certainly effectual in the cure of the violent dyſenteries ſo prevalent there, as an extract of ſaffron, which contained a large quantity of the beſt Opium*. In ſome caſes of complete dyſentery which occurred to me, while the regiment at preſent quartered here was during the indiſpoſition of the ſurgeon committed to my care, I found the method recommended by Sydenham very generally effectual. The purgative I employed was the Oleum Ricini, which appeared better adapted to the complaint than either rhubarb, or the ſaline or reſinous purgatives. The opiates were repeated generally three, ſometimes four times in the four and twenty hours, and were given in a large cup of port wine. Out of about fifty caſes treated in this way only two died, both of whom were afflicted with the complaint for a long time to a very violent degree. One of them paſſed a very large

* Hiſtoria Natur. & Med. Ind. Orientalis, lib. ii. cap. 4. p. 20.

portion

portion of the whole internal coat of the inteftine three days before he died; and in both thefe cafes, every other means recommended by different authors were tried to no effect. The fweating plan recommended by Mofely was not omitted. I imagined I perceived fome good effects, in different inftances, from adding to the opiate twenty-five or thirty drops of antimonial wine, or a proportionate quantity of Vinum Ipecacuana.

I am very much inclined to fufpect, that many authors have been led to confider Opium as either ufelefs or pernicious in dyfentery, merely from not having given it at fufficiently fhort intervals, or in fufficient dofes. Nothing, for inftance, is more common than giving an anodyne at night, which eafes and compofes the patient; and, in the morning, when its efficacy is worn out, and the tortures of the complaint again return, in place of repeating what before proved effectual, recourfe is had to fome irritating purgative. Before fentence of condemnation be paft, it would furely be

but proper to give fair trial to a medicine which has given temporary relief, and promiſes, if properly adminiſtered, a perfect cure. If we conſider, moreover, the practice of many authors who condemn the uſe of opiates in dyſentery, we ſhall find the doſes they gave infinitely too ſmall to produce any good effects. Pringle, for inſtance, either preſcribed a julep, the largeſt doſe of which contained about the ſixth part of a grain of Opium; or at night gave ſeven grains of Matthews' pill, which did not contain one grain of the ſame medicine *. The preſcriptions of many others are, in this reſpect, equally inſignificant; we muſt therefore at leaſt ſuſpend our judgment, reſpecting the utility of the practice recommended by Sydenham, until it undergoes the fair, candid, and extenſive trial it has not yet obtained. I have ſtated what has hitherto occurred in my own practice, and from it muſt ſay, I am prejudiced in its favour: but the number of

* Diſeaſes of the Army, as above quoted.

cafes are yet too fmall to form a definitive judgment, and the nature of the complaint may perhaps differ in different fituations and climates. My fentiments, however, may foon be more fixed, as the complaint is, in fome degree, endemial among the lower clafs of the inhabitants of this city and its vicinity; and I fhall omit no opportunity of examining the matter impartially.

Let all which has been faid, however, be applied to the dyfentery in its early ftages only. Where ulcerations of the inteftines have taken place, though Opium may prove ufeful as a palliative, we are not, I fear, to expect from it alone, and indeed feldom from any other medicine, an effectual cure.

TETANUS.

In this complaint it is very univerfally agreed, that Opium is the chief remedy to be relied on; and the dofes in which it is given, muft be both larger and more frequently repeated, than in almoft any other cafe what-

ever; nor do ſuch quantities affect either the mental functions or the pulſe, to the degree infinitely ſmaller doſes do in other ſtates of the body; a circumſtance as unaccountable as any attending this very extraordinary complaint.

Chorea.

In one well-marked and violent caſe of this diſeaſe, which I was a witneſs to, after electricity and every other remedy had been tried in vain, recourſe was had to very large doſes of Opium, which afforded conſiderable temporary relief; the convulſive motions, however, always again returned, and the patient at laſt fell a victim to the diſeaſe.

Epilepsy.

The pathology of this diſeaſe is ſo little underſtood, the ideas of phyſicians, reſpecting the proper practice to be purſued, ſo contradictory, and the complaint itſelf ſo ſeldom the ſubject of the phyſician's practice, that I cannot pretend to offer any thing deciſive reſpecting the employment

of Opiates in its removal or palliation. Reaſon would lead us to conclude, and experience has ſometimes proved, that they mitigate and alleviate the violence of the ſymptoms; but a confirmed cure is hardly to be expected from them.

ASTHMA.

In the pure ſpaſmodic ſpecies of this complaint, the violence of the dyſpnœa is beſt relieved by Opium; and it will be found, if properly adminiſtered, as ſafe, and more effectual, than any other article of the antiſpaſmodic claſs *.

PYROSIS.

In this complaint, not an uncommon one among the lower claſſes in this country, the paroxyſms, as Cullen obſerves, are only to be certainly relieved by Opium; its effects, however, are generally but temporary; I think it beſt removed by changing the farinacea, which thoſe who are liable to

* Cullen, Firſt Lines, ſect. 1397.

it generally feed on, for animal food. I have tried the nux vomica, recommended by Linnæus, without effect.

Colic.

The different species of this complaint vary not a little in their nature, and necessarily require an equal variation in practice. Where the constitution of the patient is such as gives reason to apprehend an inflammation of the intestinal canal, or where the disorder has been preceded by long continued costiveness; however we may be tempted, by excruciating pain or vomiting, to exhibit Opium, it should always, if possible, be deferred till blood has been drawn, and the intestines well evacuated by purgatives: where, from violent vomiting, such purgatives cannot be retained without the assistance of Opium, it is often advantageously conjoined with them, or given some time before they are taken, so as to enable them to produce their proper effect. In oth r cases of the complaint, however, not preceded by costiveness, and arising rather from

affections of the nervous ſyſtem, as in the hyſteric colic, and that ariſing from the application of cold, or affections of the paſſions, it may be ſafely had recourſe to, without previous evacuation, for the ſuppreſſion of vomiting, the alleviation of pain, and complete reſtoration of health *. In that ſpecies of the complaint likewiſe termed the Colica Bilioſa, or Colica Hyſterica, attended with bilious evacuations, after the ſuperabundant quantity of bile has been diſcharged, no medicine proves of greater ſervice. I cannot, however, help remarking, that, notwithſtanding the cautions given by the great Sydenham, and repeated by many ſucceeding phyſicians, ſeveral practitioners ſhould in this, and many other affections termed bilious, frequently perſevere to an unneceſſary degree in the repetition of emetics and purgatives. They ſeem to conſider, that the irritation of the inteſtinal canal ariſes univerſally, in theſe caſes, from

* Cullen, Firſt Lines, ſect. 1445.———Sydenham, Opera Omnia, cap. 7.

the increaſed ſecretion of bile; whereas ſuch increaſed ſecretion is very generally merely the *effect* of the continued irritation of repeated emetics and purgatives; and thus, what is intended to remove, ſerves but to confirm and continue the diſeaſe. We know to a certainty, that an irritation of the excretory duct of any ſecreting organ, neceſſarily increaſes the quantity of fluid it naturally ſeparates from the blood. We know equally well, that an emetic, taken by a perſon in perfect health, will produce an increaſed ſecretion and evacuation of bile: and thence will appear the manifeſt impropriety of perſiſting in the *repeated* employment of ſuch medicines, after the firſt collections have been removed; and the neceſſity of having recourſe, in theſe caſes, to opiates, for an alleviation of that irritation, which principally ſupports the diſeaſe, and continues the increaſed ſecretion. This is a practice agreeable to reaſon, and ſanctioned by the authority of Sydenham. Inſtead of a contrary practice, he directs anodynes to be taken: "Licet viridis ac pravus ma-

teriæ vomitu rejectæ color, contrarium indicare videatur. Subtilior enim eſt, ac minutior, colorum ſpeculatio, quam ut evacuationibus quas ipſo facto nocentes deprehendimus, auctoritatem conciliare valeat. Nulluſque dubito morbum hunc (qui licet acerbiſſinum dolorem, nullum tamen vitæ diſcrimen adportat) ob errores hoc nomine commiſſos, lethalem ſæpenumero evadere. Adde quod, ſi quis emeticum vel fortiſſimum hodie propinaverit, ut ſcilicet eam quam putat morbi mineram exhauriat, poſtero die materiem æque viridem aut pravi alterius coloris, atque erat prior illa, evomet *.

Cholera.

In this, the practice laid down by Sydenham is very generally purſued; and the neceſſity of opiates, after proper dilution, univerſally acknowledged. I met with one caſe where, in conſiderable doſes, they proved for ſome time ineffectual, the vomiting and purging continuing to that de-

* Opera, p. 215.

gree,

gree, that the patient was reduced in ſix hours to the very brink of death: his pulſe was not to be felt; his extremities were quite cold, and affected with violent ſpaſms; his features collapſed; the ſtools flowed from him involuntarily; an almoſt inceſſant ſtraining to vomit continued; the inſtant he ſwallowed a fluid of any kind, it was rejected; and opiate injections were, in like manner, ſpeedily returned. In this ſituation I gave him at once two drams of vitriolic æther, which inſtantly ſtopped the vomiting; in a ſhort time I followed it by forty drops of laudanum, which he retained, and, by a proper repetition of the opiate, recovered.

DIARRHÆA.

It need hardly be mentioned, that, for the cure of this complaint, opiates, combined with aſtringents, are the principal remedies to be relied on.

Hysteria.

Though we are not to expect from Opium a complete cure of this complaint, yet, if taken at the approach of the fits, it very generally prevents their formation: thirty, forty, or fifty drops of the tincture, mixed with any cordial water, may be given for a doſe. Vitriolic æther produces the ſame effect; but both ſeem to loſe their powers by frequent repetition, as well as the leſs efficacious fœtids ſo generally employed.

Hydrophobia.

We have had ſome accounts of very large doſes of Opium being employed in this complaint with ſucceſs; but whether any advantages are to be derived from it, experience alone muſt determine. Any ſuch expectation, however, ſhould not prevent our having immediate recourſe to the only ſure preventative, cutting out, or otherwiſe deſtroying the originally affected part.

Mania.

I fear our knowledge of the pathology of this diſeaſe is ſo limited, that the practice muſt be, for the moſt part, empirical. The increaſe of excitement which certainly takes place in ſome ſpecies of the complaint, would ſeem to prohibit the employment of any ſtimulant, and conſequently the uſe of Opium. Other ſpecies of the diſorder, however, occur, in which every debilitating power ſeems only to aggravate the ſymptoms; and, in ſuch, we ſhould ſuppoſe Opium, wine, and other ſtimulants would prove ſerviceable. Whether, in the firſt ſpecies hinted at, Opium may prove uſeful by inducing finally a ſtate of collapſe, or indirect debility, without previouſly effecting a more than adequate injury by increaſing excitement, may to ſome appear doubtful, but would ſtrike me as ſufficiently probable. The diverſity of ſpecies alluded to, does not, indeed, ſeem to have been ſufficiently kept in view by many writers who have treated of the ſubject, and appears to open

a new field to ſuch as have opportunities of future inquiry. Whatever foundation there may be for theſe ſpeculations, it is certain that Opium has been frequently exhibited in mania to advantage, allaying the fury of the enraged, and calming the agitation of the irritable. The ſyſtem, however, is as proportionately inſenſible to its operation as to that of emetics, purgatives, and every other application or impreſſion: I have ſeen fifty, one hundred, and two hundred drops of the tincture given with evident advantage to the patient, and without leaving any bad ſubſequent effects. It is extraordinary that, after the operation of an emetic, a comparatively ſmall doſe of Opium will, in ſuch caſes, produce an equal effect, as from one to two hundred, not preceded by ſuch evacuation; a circumſtance in practice to be kept in view. Emetics, to produce their proper effect in mania, muſt be generally given in uncommonly large doſes: ten grains of tartar emetic is a common doſe; ſometimes above twenty have been given; but I have heard it aſſerted, that opiates,

opiates, exhibited before the emetic, render a much lefs quantity of the latter neceffary. Of this, however, we have had no proper experience.

Syphilis.

It has been of late afferted, that Opium given in large dofes, effectually removes every fymptom of this complaint, without the affiftance of mercury. Of the trials made with it, a particular detail is given in a paper written by Dr. Michaelis, and publifhed in the London Medical Communications *. From half a drachm to two fcruples of Thebaic extract, were given in the courfe of twenty-four hours; and no extraordinary bad confequences are faid to have refulted from fuch large dofes. In the hands of others it has not proved fo fuccefsful: Michaelis himfelf confeffes, that, after trial in the different military hofpitals in America, it was, towards the clofe of the war, totally laid afide. I do not, indeed, fee what peculiar advantage can arife from fubftituting Opium for mer-

* Vol. i. art. 23.

cury; the latter, if properly adminiſtered, will ſurely hurt the conſtitution as little, and probably leſs than Opium in ſuch large doſes; and appears, from a compariſon of the different accounts, a much more certain remedy. I cannot ſpeak from my own experience, having never truſted to it for a cure. In the cure of venereal ſwelled teſticles, Opium is ſtrongly recommended by Swediam *. I have known it in ſome inſtances effectual, and in others obſerved it fail. The uſe of Opium in allaying different other ſymptoms, attendant on the various modifications of the venereal diſeaſe, or ariſing from the uſe of mercury, is well known. A repetition of particulars would be tedious.

* Obſervations, &c. p. 80.

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